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Vol. XXIII. No. 5 September, 1916

The Psychological Review

THE NATURE AND PROBABLE ORIGIN OF BINAURAL BEATS

BY JOSEPH PETERSON

University of Minnesota

If two tone waves of slightly different pitch are conducted separately in tubes, one to each ear, it will be found that they beat. Beats of this kind are called binaural to distinguish them from beats produced by the operation of both tone waves in the same ear. A question of importance to auditory theory arises here: Are these two kinds of beats, binaural and monaural, really different in principle? Are binaural beats not due to some sort of conduction of the vibration series from one ear to the other so that both waves really operate in each ear? At least three modes of such conduction from one side to the ear at the opposite side of the head are possible. Unless much care is taken in connecting the sound-tight tube to the meatus of the ear, vibrations may be communicated to the air external to the head and thus carried to the opposite ear as an ordinary air wave. As a second possibility the tone wave may go directly from the air in the meatus to the bones of the skull and thence to the liquids of the inner ear on the opposite side of the head. Finally, some of the middle ear structures, or even the liquids of the labyrinth, may communicate the wave to the bones of the skull by which they can be conducted to the opposite ear. It is conceivable, moreover, that under certain conditions all these means may be operative.

Evidence is not wanting to show that such cross-conduction of the wave is probable. It is well known that vibrations are effectively conducted to the labyrinth by the bones of

the head. This is illustrated by the Weber test. If the stem of a sounding fork is held against the skull at the right ear, or at any other place, while the meatus of the left ear is plugged with the finger tip, the tone will be clearly perceptible in the left ear even though the vibration of the fork is very weak. Auscultation of the skull roof by means of a sensitive microphone has shown that the bone may take up the wave.[1] Wilson and Myers report[2] that by leading the tone of a tuning fork to one ear of an individual they 'proved that an appreciable amount of sound could be transmitted through his head to the other ear and could thence be conducted by a tube so as to be audible to the ear of a second observer.' It is well known that even inaudible interfering tones may produce clearly perceptible beats. Therefore it is not safe uncritically to assume that faint tones are not transmitted to the opposite ear. S. P. Thompson found, for example, that two Ut_3 ($c' = 256$ v.d.) forks, slightly mistuned, produced very distinct beats when their tones were conducted in tubes one to each ear even though the tones were so faint as to be 'all but inaudible' when sounding individually.[3] For these reasons, and others to be considered later, there is a tendency on the part of certain investigators to attribute all binaural beats to interference of vibrations in the same labyrinth, due to bone conduction.

Certain experiments have produced evidence of a peculiarly forceful kind against this interpretation of binaural beats. Cross and Goodwin[4] in a carefully conducted investigation attempted to exclude cross-conduction of waves. That they were entirely successful in this attempt is not here maintained. Rods connected with the stems of the sounding forks were held against wax plugs in the ears. Binaural beats were plainly audible even when the tones were very weak, but difference tones could not be obtained, with the necessary pitch difference of the forks, though the intensities were greatly increased. If, however, one of the rods was

[1] Nagel, 'Physiologie des Menschen,' 1905, 3, 575. References are there given.
[2] *Brit. Jour. Psychol.*, 1906, 2, 381.
[3] *Phil. Mag.*, 1877, (5) 4, 274–276.
[4] *Proceedings of the Acad. of Arts and Sciences*, 1891, 27, 1.

held against the skull or the teeth while the other was in contact with the wax in one ear "a loud differential tone was heard at once in the ear against which the rod was placed." In such a case both the vibration series could operate together in the one ear by means of bone conduction from the rod touching the skull. The intensity of the beats did not seem to be affected by this change, however, as we should expect if the beats were also due to the action of both waves in each ear. This experiment certainly suggests that bone conduction was not very strong, if indeed it was possible at all, until one rod was held against the skull. Difference tones are possible only when both tone waves operate in the same medium either at the source of the tones or in the ear. This general statement of the conditions under which combination tones are possible is, I think, agreed upon by all authorities irrespective of bias as to any particular theory of the origin of such tones. This is one of the strongest pieces of evidence against the view that binaural beats are to be explained on the basis of bone conduction, yet no one maintaining this view has considered seriously, or at all, it seems, the results of the experiment here mentioned.

Binaural beats have been studied incidentally of late by a number of experimenters who were primarily concerned with the perception of phase differences of waves at the two ears. These investigations have been carried on mostly by physicists under conditions so carefully controlled that there can be little room for differences of opinion as to the general results.[1] It is now unquestionably established that phase differences are perceived, but the explanation of the whole matter is still in controversy. Wilson and Myers maintain that such perception is after all based upon intensity differences at the two ears, and they have explained in an ingenious manner how all phase differences at the ears may conceivably be converted into intensity differences with the consequence that the greater intensity occurs at the ear receiving the

[1] Rostosky, *Phil. Stud.*, 1902, **19**, 557. Lord Rayleigh, *Phil. Mag.*, 1907, **13**, 214, 316. More and Fry, *Ibid.*, 1907, **13**, 452. Bowlker, *Ibid.*, 1908, **15**, 318. Wilson and Myers, *Brit. Jour. Psychol.*, 1908, **2**, 363. More, *Phil. Mag.*, 1909, **18**, 308. Stewart, *Physical Rev.*, 1914, **3**, 146; **4**, 252. Schulze, *Annal. d. Physik*, 1914, **45**, 283.

earlier phase. Their objection to the view that phase differences are directly perceived, as Lord Rayleigh, More, and others have assumed, is that it compels us to depart radically from our present conception of the origin and nature of the nervous impulse. "We have hitherto believed that, under ordinary circumstances, all sensory stimuli (save, possibly, painful ones) act on the nerve fibres solely through the intermediary of end organs; and that these organs may in certain cases be excited by various kinds of stimuli—mechanical, electrical, chemical. Since these diverse kinds of stimuli give rise to similar sensations, 'we have hitherto believed that the impulses ascending a sensory nerve depend on the mode of response of the end organ and not directly on the character of the stimulus.'[1] "It is very hard to believe that every crest and every trough of each sound wave produce an exactly corresponding crest and trough in the impulses transmitted along each auditory nerve."[2]

The theory of Wilson and Myers is based on three suppositions: (1) That the sound wave is transmitted to the opposite ear by bone conduction; (2) that the retardation of phase due to this ear-to-ear transmission is small; (3) that the two waves, the direct and the transmitted, arrive in each of the cochleæ from opposite directions. These suppositions, which are crucial to the theory, have been questioned, under certain conditions, at least, by More,[3] who has repeated the experiment of Wilson and Myers. More also tried higher-pitched forks than any of those used by these experimenters—128, 180, 240, 256, 320, 384, and 512 v.d. He found that 512 v.d. was near the limit of accurate lateral location by phase differences. Above this the accuracy decreases, becoming untrustworthy at about 1,024 v.d. No systematic tests for continuous pitch differences above 1,024 v.d. were made, "yet a qualitative trial [under the accurate conditions given by the use of the Wilson-Myers apparatus, however] with a fork of pitch 3,000 v.d. approxi-

[1] *Op. cit.*, p. 377.

[2] *Ibid.*, p. 378.

[3] More, L. T., 'On the Localization of the Direction of Sounds,' *Phil. Mag.*, 1909, 18 (6th Series), 308–319.

mate, showed that absolutely no sensation of direction existed."[1] More reports, moreover, that tones "so feeble that sufficient sound did not travel through the head to produce any sensation" were as easily localized by phase differences as were loud tones.[2] Suddenly pinching one of the tubes, thus instantaneously decreasing the intensity at one ear, did not, as would be expected on the Wilson-Myers theory, cause a confusion in the judgment. Extreme changes do, of course, cause a confusion, but very slight ones should do so if the perception of phase difference is reducible to perception of intensity difference. More also points out the obvious fact that according to the Wilson-Myers theory tones should be more accurately located than noises, "while the contrary is proved by experiment." With a subject completely deaf in the left ear More found that a tone, even though intensified by a resonator, could not be heard in the right (good) ear when conducted to the left one. This is strong evidence against the cross-conduction of a perceptible amount of vibration through the head.

My own experiments in acoustics, at first repeating the experiments of various investigators, early led me to doubt the explanation of binaural beats, and of the perception of phase differences, based upon cross-conduction of the wave through the bone. That such conduction is not only probable but actual under certain conditions, no one can deny. Repeated tests which I have made chiefly with forks c′ (256 v.d.) and a′ (435 v.d.) show that the perceptibility of vibrating forks held against the skull on different places varies considerably with equal intensity of vibration. A fork, *e. g.*, which becomes inaudible on almost any other part of the skull may again be heard if held against the mastoid process behind the ear. The sensitivity to a fork at this point is almost equal to that of one held against the teeth, yet when the fork has 'run out' at either of these two positions it is easily perceptible when held with the prongs just before the ear as closely as possible without making actual contact.

[1] *Op. cit.*, 314.
[2] *Ibid.*, 318.

If now, when the fork is held against the mastoid process, a finger tip is inserted into the opposite ear it is found that the tone is not only located in the stopped ear, but that it is perceptible in this ear with fainter intensity than in the unstopped ear nearer the source of the sound. This high degree of sensitivity of the opposite (stopped) ear has been used as evidence that binaural beats are really due to bone conduction; *but such a view overlooks the important fact that the tone waves so easily perceived in the plugged ear do not come to the cochlea directly from the stimulus through the skull, but are reflected back from the finger tip and enter the ear through the usual channel.* Wilson and Myers found that if a tone is conducted to the subject's right ear, for example, a person at the opposite side might hear the tone through a tube connecting his own with the subject's left ear. This experiment obviously does not prove, as it has been thought to prove, that cross-conduction waves are easily perceptible in the opposite ear when not stopped or plugged, *i. e.*, under the conditions of the Wilson-Myers experiment proving the perception of phase differences.

Binaural beats, of the peculiar quality soon to be noted, may be heard with both ears unstopped, or normal, even if the feebly sounding forks are not even in contact with the head, but are held before the ears. The fact is, therefore, that the ear is insensitive to a high degree to tone waves coming directly from the skull, and highly sensitive, on the other hand, to those which come through the usual channel from the meatus. In the case of the plugged ear the waves from the skull are perceived, for low intensities, only when reflected back from the direction of the tympanum by the finger tip.[1] *This fact is in direct opposition to the third of the Wilson-Myers suppositions*, which can therefore be valid only in a small degree and then merely for intense vibrations.

It was the nature of binaural beats that led me to discredit the cross-conduction theory of their origin, much as the theory was consoling to my own view in a time of per-

[1] Cf. Mach, *Berichte d. Wiener Akad., math.-nat. Kl.*, 1863, **48** (2), 283, cited by Schaefer, K. L., in Nagel, *op. cit.*, 574.

plexity. On the nature of binaural beats I have gathered—as a control test to my own observations—unbiased descriptions from a number of competent observers. Forks sounding with as nearly equal intensity as possible were held in irregular, or chance, order either one at each ear or both at the same ear. The forks—c′ in this case—differed slightly in pitch so as to beat very slowly, approximately once per second, and the observers were asked to write out descriptions of the beats. They were at the time ignorant of the use to be made of the report and were not informed on this matter until quantitative tests had been made, following the descriptive reports, to determine the accuracy of the counting of binaural beats. The results here given are only those obtained from experienced observers, instructors in psychology, and are in substantial agreement with those obtained from less experienced persons. The observers will be designated as *Kr*, *D*, *Kg*, *W*, *E*, and *J*. All did not take part in every test.

Kr describes binaural beats as constituting a 'unified experience with fluctuations from side to side.' "Monaural beats are discrete experiences. There is a series of wide gaps which seem to break up the total sense experiences. These gaps are of varying size," with different relative and absolute intensities of the primary tones. There is no uctuation in the location of the beats.

E says: "Monaural beats are much more distinct and more easily counted, definitely localized, and do not shift as do the binaural beats." In counting there was 'less effort of attention required and no doubt, but rather a feeling of certainty' as to the accuracy of results. "Binaural beats less distinct, feeli g of effort clearly present in my trying to be sure of the count; slightly unpleasant; localization not as definite as in case of monaural beats; beats are often localized at one side or the other, but also very often at a fairly large region around and over the head. The sound seemed to fill this region, about two feet in diameter; as the phase changed the sound seemed to run to the opposite side across upper back part of the head, occasionally fairly vivid visual

images of narrow bands of light going back and forth in this way."

D, whose right ear is partially deaf, had extreme difficulty in counting binaural beats, and was therefore excused from the beat counting tests. The sound was continuous at the good ear, and while there was no shifting of the sound he experienced slight beating effects which he counted as slower (in the ratio of about 2 to 3) than the beats which actually occurred. For this observer the fork at the right was held nearer than the one at the left ear.

The results of *Kr* and *E* are typical and agree well with those reported by other investigators. For example, Cross and Goodwin report that two forks were held against the teeth until their beats (4 per second) were no longer audible when suddenly they were removed "and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats."[1] The beats in this case were counted correctly, as is usual with frequent binaural beats. As the beating rate increases the shifting from side to side, becoming too rapid to follow, gets to seem more and more like true interruptions, as with monaural beats. It is also evident that if there were here a small amount of cross-conduction, contrary to the opinion of the experimenters, the crossed waves would not likely produce directly a perceptible effect in the cochlea, but would be reflected back to the cochlea from the meatus along the usual channel of auditory sound waves. In such a case the beats would be monaural in each ear and would be easy to count.

For some time my experience has shown that binaural beats, within certain limits, become easier to count as their frequency increases. Very slow beats, especially of low pitched forks, are readily perceived to be located now on one side and now on the other, and to shift with the change of phase. As they increase in frequency the effect is to pro-

[1] *Op. cit.*

duce something of a circular motion very hard accurately to count, and at a still greater frequency this shifting approaches more nearly the nature of the interruptions of monaural beats, with no changes in location. The point of greatest error in binaural beat counting seems from my own observation to be at a frequency of about one beat per second, at which point the swing to each side is often counted by the unsophisticated as a separate beat, thus giving the result of multiplying the actual beats by two. In my first experiments precautions were taken to have the tones separately conducted to the two ears in tubes. The end of each tube was wrapped with a handkerchief and made to fit into the ear in such a way that the air wave could presumably not escape and enter the opposite ear by passing around the head. Tone waves were also separately conducted to the ears by means of metal rods or heavy wires each held in contact with a particle of wax in the meatus of the ear to which it led. The wax thus forms a resonating cavity in the meatus. The stems of the vibrating forks were held against the farther end of the rods. With moderate intensities the results under these conditions were precisely like those obtained by holding unresonated forks directly to the ears. Consequently the more elaborate methods were abandoned for the simpler one so that such conditions as the relative intensity of the tones could be better controlled. Another objection to the wax-in-the-ear method is that the wax stopper will reflect back the cross-conducted wave—if there be any—and make both waves enter each cochlea from the same direction, whereas we are endeavoring to find out what takes place under normal conditions when there is no such reflecting back of the crossed waves. Below[1] will be found results obtained with ears stopped as compared with ears normal, the stems of the forks being held against the skull in each case. These results amply justify the abandonment of the wax-in-the-ear method, valuable as it proves to be in certain studies.

The following results were obtained with two unresonated c′ forks of slightly different pitch. These were held in

[1] See pages 345, 346.

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The following results were obtained with two unresonated c′ forks of slightly different pitch. These were held in

[1] See pages 345, 346.

chance order a short distance before the observer's ears either both to the same or one to each ear. Precautions were taken both in actuating the forks and in the manner in which they were held to the ears to insure a practical uniformity of experimental conditions. The experimenter sat directly behind the observer, and followed a chance order of experiments both as to the frequency of beats and as to the method of stimulation just described. The order was the same for all observers. Because of the defectiveness of *D*'s right ear he was not used in this beat-counting experiment. *W*'s right ear was slightly defective, so the fork at this side was always held closer than that at the left. The observer watching the second hand of a watch began counting the beats at a favorable point and continued for ten seconds. *W* used a stop-watch, and began each time by the more accurate method of 0, 1, 2, etc. The other observers began counting with 1. No deductions for this were made as the results are wholly for comparison between monaural and binaural beats. The beats recorded are the total number counted during the ten seconds.

The pitch of one of the forks was changed, as the program required, by the shifting of a small brass clamp which was attached to one of the prongs. Three marks were made on the prong so that the number of beats for each position of the clamp would be as nearly constant as possible. The slight variations in the counts of each observer of monaural beats are unquestionably to be accounted for in part by the impossibility of putting the clamp at exactly the same point each time for a certain frequency. Even when the chance order required no change in the position of the clamp a change away from and back to the mark was made so that "O" would remain ignorant as to the number of beats to expect. Occasionally it was desirable to set the clamp in an unusual position, and get an unusual frequency of beats, so that "O" would not learn the three particular frequencies to expect. This was usually unnecessary, however, as the results for binaural beats were so variable.

In the tables which follow *N* represents the rapid fre-

quency, about 24 in the ten seconds; n, about 11.5 in the same period; and n', about 3.25. Question marks indicate a high degree of uncertainty on the observer's part. In the cases indicated by footnotes unsuccessful attempts to count the beats were made. Usually these were in immediate succession; occasionally some cases of monaural beat counting or short periods of rest were interspersed. Other features of the results are self evident. A little practice was given each observer before results were tabulated.

	Observer *E*						Observer *Kr*					
	Binaural			Monaural			Binaural			Monaural		
	N	*n*	*n'*	*N*	*n*	*n'*	*N*	*n*	*n'*	*N*	*n*	*n'*
	21	10 ?	6 ?	24	12	4	24	24	30*s*?	25	13	4
	21	10	6 ?	24	12	4	26	20	22	24	13	4
	22	12	6	24	12	3	27	20	30 ?	23	12	3 ?
	12*s*[1]	8 ?	6	24	12	4	24	28	25 ?	26	12	4
	16	12	8 ?	24	12	4	27	24	23 ?	24	12	3
	20	6 ?	6 ?	24	12	4	19	11 ?	19 ?	27	12	4
	9	9	4	24	12	4	38	17	13	25	12	5
	24	10*f*	4	24	12	4	25	11 ?	28*f*?	25	12	3
	20	9	4	24	12	4	29	20 ?	14	25	12	4
	22	10	4	24	12	3	25	20	10 ?	25	12	4
M....	18.7	9.6	5.4	24	12	3.8	26.4	19.5	21.4	24.9	12.2	3.8
M.V..	3.8	1.3	1.1	0	0	0.3	3.2	3.9	5.9	0.7	0.3	0.6

	Observer *W*						Observer *Kg*					
	Binaural			Monaural			Binaural			Monaural		
	N	*n*	*n'*	*N*	*n*	*n'*	*N*	*n*	*n'*	*N*	*n*	*n'*
	20	5	?*t*	25	12	4	8	5	8	25	13	4
	22	4	2*s*?	25	11	4	6	3	7	25	12	3
	24	6*s*?	2	24	12	3	12	9	10	23	12	4
	24	9	2 ?	25	12	3	7	6	8	25	12	4
	25	2	1	25	12	3	22	10	5	24	11	3
	25*f*?	4	1*t*?	25	12	4	7 ?	3 ?	4	24	12	3
	22*t*?	?*t*	1*t*?	25	12	3	4 ?	8	4	24	12	4
	25	2 ?	?*t*	25	12	3	23	8	3	24	12	4
	26	?*t*	?*t*	25	12	3	16	3 ?	3*s*	24	11	4
	22	?*s*	2+*t*	25	12	3	11*s*?	7	5 ?	25	12	3
M....	23.5			24.9	11.9	3.3	12.6	6.2	5.7	24.3	11.9	3.6
M.V..	1.6			0.2	0.2	0.4	5.6	2.2	2.0	0.6	0.4	0.5

[1] *s* = second attempt (the first being unsuccessful); *t* = third attempt; *f* = fourth attempt; ?*t* = failure after third attempt. Note that averages and deviations are of little value in the binaural beats because of uncertainties, failures, and in some cases marked improvement on the slowest beats, the latter due, likely, to some sort of association with the monaural slow frequencies. Fractions are omitted in the observers' reports. When fractions were given the next higher whole number was recorded if the fraction was as large as .5.

W's defective right ear seems to have made his perception of binaural beats very uncertain. In the cases of failure *W* declared that he heard beats but that they were not sufficiently distinct to be counted. It is probable that, even though the fork at the defective ear was held nearer than was that at the good ear, the relative intensities for *W*'s hearing were after all not the most favorable. No complaint of this was made however. *Kg* mentioned several times, when he gave small numbers for the frequent binaural beats, that he heard also more rapid beats. *E. g.*, when 16 was given: "I heard also a slower and a faster beating; don't know whether I gave the right one." As a rule the observers expressed much lack of certainty as to their results on binaural beats in general; question marks indicate only the places of greatest doubt.

Some peculiar individual differences are noticeable in the results. *Kr* shows a tendency to be influenced considerably in the more uncertain judgments by the easily perceptible beats in the rapid series. Frequently he reported for the slow beats: "Too fast to count." This tendency was so strong that in the practice series, which by chance began with the most frequent binaural beats, he 'counted' the slowest monaural beats as 22, 12, and 28. He declared that they were 'practically continuous.' It was found necessary to warn him that some of the beats were very slow. The next count on the slow monaural beats was still wrong, 28 instead of 3, but when the second one came round he gave a triumphant smile, and made no errors after that time. But this discovery of slow beats seems to have had no effect on the binaural slow series. It is interesting that the experience with difficult frequent binaural beats so toned up *Kr*'s expectation that his attention was wholly diverted from the very slowly changing tone bearing the monaural beats of approximately .4 per second.

The tendency which my earlier experiences had led me to expect, to count the swing to each side of the one-beat-per-second monaural series as a separate beat, is evident in the results of only one observer, *Kr*, and even these results

may be due to association with the more rapid beats which happened, unfortunately, to have just twice the frequency of the middle series. Of course the 'influence' may also have been in the opposite direction. At the frequency of approximately one per second, binaural beats seem to be a continuous sound wandering about in an ellipse, and one can subjectively ascribe one of several rhythms to them, as to the ticks of a clock. Extremely cautious observers are therefore likely to let many 'beats' go by uncounted, and to have grave doubts as to the accuracy of any of their results. This continuous nature of the sound, even for weak tones, in the case of slow binaural beats speaks strongly against an even indirectly appreciable amount of cross-conduction from ear to ear. The results of the trained observers do, however, show, as was expected from the earlier experiments, that of the three frequencies used the beats of approximately one per second are the most difficult accurately to count. It is at this frequency that inexperienced observers seem to be most likely to count each side shift of the tone as a separate beat, thus making the error of multiplying the actual number of beats by two.

As a rule beats are easily counted when the stems of the forks are held against the skull, if the vibration is intense or even of medium intensity. If the forks are pressed hard against the skull the beats become more distinct and approach the nature of monaural beats. The shifting of the sound from side to side is more noticeable when the forks are held one at each side of the cranium than when they approach the median plane. If the intensity is low and the forks are held against the skull near the ears, one at each side, the counting of the beats becomes very difficult and uncertain even though both tones are clearly audible. A test on Mr. *E* shows counting under these conditions to be almost entirely guesswork. The results are more variable than his counts of binaural beats given above; *e. g.*, beats at a frequency of 1.2 per second were counted in successive trials (other frequencies being interspersed, as explained in the foregoing, to keep the observer in ignorance of the frequency used) as

follows: 4.5, ?, 12, 9, 3. The stems of the forks were held against the mastoid processes behind the ears. Here again the rapid beats were more accurately counted. The full table of results is not given because it was extremely difficult to control properly and keep constant both the intensity of each fork and the degree of pressure on the skull. Even when the experimenter could hear the beats plainly with the forks 45 to 60 cm. away the observer was unable to count them correctly. I was surprised several times when the beats seemed intense that the observer did not get them by air conduction around the head. On the contrary he pronounced the experiment 'harder than any I have had.' This experiment shows how much the possibility of air conduction around the head has been exaggerated by some writers. Caution, however, where there is uncertainty is a good thing.

This experiment was repeated with the change, that the observer was asked to put a finger tip into each ear. Under this condition *E*'s results with ten trials on each of the three frequencies of beats shown in the tables above were precisely of the same degree of accuracy as was his counting of monaural beats. In thirty-five trials not an error was made. *E* described the beats as very distinct and clear. It was evident that we now were getting monaural beats in each ear. The location of the sound did not shift, as it does with true binaural beats. Since there is no reason for the existence of more bone conduction with the ears stopped than with them open, it appears *that vibrations may be sent across the skull from ear to ear with a considerable degree of intensity and yet not produce beats in the opposite unstopped ear with another tone.* The auscultation experiments, which have been used to prove the effectiveness of bone conduction in binaural beats, and in the location of sound by phase differences at the two ears, therefore prove nothing in this regard. On the contrary, it is highly probable from the results here given that the effect of such cross-conduction is entirely negligible.

Convincing as some of these results from normal observers may seem to be against any explanation of binaural beats

that assumes bone conduction from ear to ear, it is still desirable, if we are to avoid any possible error in our conclusion, that a study of bone conduction be made on a monaural observer. More's experiments were carried out in part on such a person, and confirm our results perfectly. "Miss *S.*, after an attack of spinal meningitis at the age of four years, entirely lost the sense of hearing in the *left* ear. Examination by aurists showed that the nerve of this ear was atrophied, and the power of hearing gone." The good ear, we are told, 'was abnormally acute.' More found that when the right (good) ear and the tube leading to it were stopped with cotton no sound was heard. The result was not different even when the fork, whose tone was conducted to the ears, was intensified with a resonator. That is to say, a loud tone conducted only to the deaf ear was not audible in the good ear by bone conduction even though the latter was plugged with cotton. Beats may, however, be audible even though the primary tones interfering cannot be heard. Could *S* have perceived beats if moderate tones of slightly different pitch were conducted separately one to each ear? No such tests seem to have been made.[1]

Before seeing the report of More's experiment I had planned independently more extensive tests on a monaural subject. The experiment was carried out somewhat over a year ago in the psychological laboratory of the University of Chicago. An observer, *J*, was found who is entirely deaf in the left ear both for high and for low tones. This condition resulted from an attack of scarlet fever in childhood. The right ear is apparently even better than the average in its acuity for tones. An unresonated fork of 768 v.d. was used. With a certain intensity (determined by the impact actuating the fork) which made the tone just audible at a distance of approximately 60 cm. from the good ear, no sound was perceptible at all when the fork was held at the left side of the head. With greater intensity of vibration the fork on the left side was audible in the right ear, and vaguely localized on the right side. At 90 cm., for instance, the fork was thus

[1] More, *op. cit.*, 315, 316.

heard. In this case the air wave had to go around the head. As the fork was now brought nearer the deaf ear, with the same intensity of vibration, the tone became inaudible for a considerable distance and until the fork came very close to the ear, when it was again feebly audible. This inaudible zone on the left side of the head was established by repeated tests for which the fork was moved in both directions, both toward and away from the ear. If now two beating forks were held at the left side of the head, one just within the perceptible limit and the other just inside the inaudible zone, beats were at once heard. These beats were localized vaguely outside the good (right) ear. That they were monaural in character is evident from the fact that with very slow beats complete interruptions of the tone were produced between each two successive beats, that is, there were intervals, as is the case with monaural beats from extremely weak tones, during which no sound was audible at all. No wandering, or side to side shifting of beats was perceived, moreover, when one fork was held at each side of the head.

It was thought that this beating might still be due to cross-conduction of the wave through the skull, so a more carefully controlled set of tests was planned. These experiments were carried out with unresonated c′ forks, 512 v.d. *J* plugged the good ear with his finger tip while the fork was held at the deaf ear. Several intensities were tried. Under these conditions no tone was heard at all even when the intensity was sufficient to make the tone audible normally at a distance of 9 or 10 meters. With the right ear plugged as just described and the stem of the fork held against the skull the tone was at once perceived, now located definitely in the right ear. It became inaudible when the tone became so weak as not to be heard by the normal ear at a distance of 60 cm. This intensity, with the opposite (good) ear plugged, it should be noted, is considerable. The measurement is important, as it is impossible on normal subjects. Let it be noted also that *J* is an usually reliable observer. In this measurement the stem of the fork was held against the mastoid process behind *J*'s deaf ear. When the right

ear was plugged and both forks at a slightly different pitch were held before the deaf ear no beating was heard whatever. This is significant for, as has been said above, even inaudible tones may be heard to beat. If now one fork was held with the stem in contact with the mastoid process behind the deaf ear there was still no beating though the fork was itself audible. Beats were at once remarked when the second fork was also held against the skull. No variation in these results was obtained when the tones were conducted separately, one to each ear, either in tubes or along rods as described in the early pages of this paper, *i. e.*, the results were like those obtained by holding the unresonated forks to the ears. Two tones thus conducted to the deaf ear were inaudible and did not beat.

These experiments with a monaural subject, then, agree in a striking manner both with those conducted by More under different methods and with those here reported on normal observers. In every case the results are directly against the view that binaural beats and the perception of phase differences are to be explained on the basis of conduction of the sound wave from ear to ear through the skull. We seem to have no choice but to conclude that binaural beats are distinct both in character and in physiological cause from monaural beats. The latter are more discrete than the former, do not wander or shift from side to side, and may involve complete interruptions in the audibility of the beating tones. If the tones conducted separately to the two ears are unusually intense there may possibly be interference in each ear due to bone conduction of each tone to the opposite ear, where it will interfere with the other tone. In such cases beats are of a dual nature, being partly monaural and partly binaural. Such cases, if they ever occur, are extremely rare. Pure binaural beats are evidently not beats at all in the usual sense of the term as used in acoustics; they are periodically perceived changes in a tone whose location wanders or shifts from ear to ear. As these shiftings become rapid the effect is that the perceived changes in the tone approach more nearly the character of true monaural beats, and may be counted with more precision and certainty.

Binaural beats and the perception of phase differences are evidently to be explained on the same principle. Both of these phenomena seem to be cortical in origin, while monaural beats probably originate in the basilar membrane. Combination tones, both 'subjective' and 'objective,' are in all probability objective in origin to the basilar membrane. For the production of 'subjective' combination tones both waves must operate in the same ear, where each periodically modifies the intensity of the other.

The conclusion to which we are here driven by facts, that binaural beats are but the shifting of the location of the sound from side to side, and are not true beats, may not, after all, violate so seriously as Wilson and Myers suppose our present conception of the nature of the nerve impulse. It is well known that the mere passage of an electrical current through the nerve fiber does not excite it. Only changes in the current, whether increasing or decreasing it, excite the nerve. Up to a certain limit the more sudden the change the greater the effectiveness of the stimulus. Oscillations in a very intense current will continue to excite the nerve up to a frequency of 1,000,000 per second.[1] The nerve fiber has a very short 'refractory period' during which it is inexcitable by another stimulus which follows the first. The length of this period varies with different nerves, and seems not to exceed .002 second. Just how short the period may be in a sensory nerve such as the auditory nerve, or rather the fibers of the cochlear branch of this nerve, is not easy to determine. There is evidence, moreover, that the refractory period of the nerve is conditioned by central rather than by peripheral factors.[2]

There seems, then, to be ample basis in fact for a theory that the perception of phase differences at the two ears is due to some central factor. Let us recall that visual perception of distance and direction rests on numerous impulses not directly in consciousness; that these several impulses in many cases have no peripheral connections whatever and

[1] Ladd and Woodworth, 'Elements of Physiological Psychology,' p. 131. References are here given.

[2] *Ibid.*, p. 165.

come to the cortical centers through different sense channels; and that the organism by the trial and error method of procedure gets gradually and unconsciously to 'take note' of them in its responses, thus learning to locate objects. It is not inconceivable that by a similar process the organism comes gradually in its responses to take note of certain central differences in the 'phases' of the streams of impulses from the two ears. It is hardly necessary in our present day in psychology to remind the reader that the organism as a whole is after all the responding or reacting agent, not consciousness. This view is consistent with our present behavioristic tendencies. In fact I see no alternative but to accept some such a theory of central perception of phase differences as here suggested. Our theories must follow the facts. At present this view does not seem to be incompatible with any of the more general theories of hearing whatever the mode of analysis in the ear may be assumed to be.

In closing I wish to express my thanks to the instructors in the University of Minnesota who served as observers, and to Professors James R. Angell and H. A. Carr for their assistance in carrying out the experiments on the monaural subject.

THE INTELLIGENCE EXAMINATION AND EVALUATION

A Study of the Child's Mind

(Second Report)

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New York

I. Introduction

In this second report on the Intelligence Examination and Evaluation, I shall try to take up the various faculties of the child's mind, not in a critical but rather an interpretive way, and explain quite fully the method of examination and evaluation by means of the tests and test-sheet published in my previous paper.[1] This study I trust will not only serve in the testing and understanding of the normal child,[2] but will especially give the psychoclinical point of view—that is, that of the trained physician who is likewise a trained psychologist—for the discernment and comprehension of the atypical and abnormal one.

Our criticism in the previous paper, based upon extended theoretical study and practical experience, led to the conclusion that the Binet method is utterly inadequate to furnish one with either an accurate or truthful equation of the general intellectual ability of an individual, and that, furthermore, an equation of the *general* ability—even if that were obtainable by this method—is of no diagnostic value whatsoever to the psychopathologist or psychoclinicist, for in mental pathology disease processes involve not the *general* ability (neither the brain generally nor the mental

[1] 'The Intelligence Examination and Evaluation and a New Intelligence Examination Sheet,' *J. of the A.M.A.*, July 31, 1915.

[2] Our revised sheet also contains more difficult tests, so that the sheet may be used in examining adults too.

functions generally) but indeed operate very electively—at times most astonishingly so.[1] Drugs and poisons act similarly. Besides, in that group of cases known as *psychopathic constitutions*—that group which is largest in number, most important because most amenable to treatment, and most interesting because it embraces many ingenious personalities or individuals of talent, so many, in short, who, be they more or less (*or not at all*) intellectually deficient or defective, are free individuals in their community or in society, with strong potentials for good, and especially for bad, whose life tenor from the earliest may be seen winding both in and out the highway of normality, and who are not aberrant by reason of affected intelligence, but because their leverage of mental functioning is out of gear—in this large and immensely important group, Binet tests bring out nothing and test nothing, and merely mislead to false conclusions.[2]

Our mental and intelligence examination, if it is to be of practical value, must supply us with data that can serve in diagnosis, prognosis and prophylaxis, and that shall give us a clew to therapy and remediable pedagogic procedures. It is readily to be seen that an equation of the general intellectual ability (again to say, if this were obtainable) can of course in no wise fill this requirement, not even approximately. With our procedure (and sheet) no such equat on is therefore sought; we test separately the so-called faculties of the mind and several additional psychological processes influencing these faculties, and scrutinize singly the data obtained before summing up, just as the physician must note his findings in examination of heart, lungs, kidneys, etc., etc., singly before collating. For only by keeping the condition of these individual organs *disjointly* in mind can he finally survey his case *jointly*, and come to a diagnosis.

Our method, then, does not methodically apply any *scale*, nor give you any *general* grading; it affords one, after the application of the tests, an understanding of the tested individual's memory, knowledge, comprehension, combi-

[1] See G. Anton, 'Jurist.-psych. Grenzfragen,' Halle, 1910, p. 5–6; also Th. Ziehen, 'Die Geisteskrankheiten des Kindesalters,' Berlin, 1915, p. 69–70.

[2] For the literature on these types see preceding article.

national ability, and finally his attention, feeling, and reliability of memory.[1] If his memory is found poor, it may explain the lack of knowledge in such individual, for knowledge is only possible with a normally-functioning memory. If attention is strongly involved, this may explain a poor memory; for, in order to remember, the attention must hold long enough to have the process of memory register a series of concepts. Or again, focusing clinically, if a separate faculty, as that of combination, is found defective, we immediately think of one of those psychoses (as dementia præcox) in which this faculty is the first to show invalidation, etc.

But mental pathology has taught us the necessity of even refining our examinations more minutely, of testing memory, for instance, not grossly, but as to kind and time; *i. e.*, immediate, recent and past memory, memory of the heard, the seen, of language, numbers, color, form, etc.; of testing comprehension, not as such generally, but the comprehension of concrete ideas, abstract ideas, of numbers, differences, time, form, pictures, etc. In the Korsakoff syndrome,[2] for example, in which the patient seems to have lost all sense of time, place, and recent events, careful study has elicited the following: The sense of time is in no wise impaired, notwithstanding how evident and positive this appears on clinical observation; there exists, however, momentary disorientation, the patient remembering nothing or little of the recent, though past memory and the knowledge and experience derived from that memory remain quite normal.[3] Things told him at the moment can be repeated (*i. e.*, his immediate or rote memory, or, as one might term it, *impressibility* of memory, is intact), but the minor associations which go with such immediate registrations and localize them in time, and which are so all-important for our momentary orienta-

[1] Out of which stuff the intelligence is made. (See previous paper.)

[2] A chronic mental condition (mostly, though not always, due to chronic alcoholism) characterized by disorientation, irregularly impaired memory and confabulation, commonly associated with multiple neuritis.

[3] But there may also occur a retro-active amnesia, going considerably beyond the period of the illness itself.

tion, are not retained; and hence, too, all idea of sequence is lacking. To cover over these immediate and recent memory lapses (as well as the time involved in the retroactive amnasia), the Korsakoff patient fabricates prodigiously —confabulates, as we say; and these ever-changing confabulations serve him for pseudo-memories and save him embarrassment, and leave him, in fact, remarkably unconcerned.

The interesting studies on this syndrome by Gregor[1] in recent years also showed that through continuous repetition and practice the impressibility may be increased and 'widened,' and thus the memory defect improved.

Here, then, you will observe how through finer and more detailed memory examination and study, the patho-psychology of this psychosis was unravelled, and even a helping therapy found.

Other examples could be given, as that of paresis, or the illuminating aphasia group. But the above will surely suffice to confirm that crass testing will never evolve fine criteria to diagnosis, and that only by fuller and *detailed* examination will one ever clear up complicated entities, or come upon clews to therapy. Are we sure that the pathology involved in the case of a child that does not 'get on' in school, or the one brought to us with the home-made Binet diagnosis of 'defective' or 'feebleminded'—the diagnosis that every college girl now attempts, and that is being made in all our clinics, and at the children's courts, chiefly by non-medical psychological workers (and some equally well prepared medical ones, too!) who have had no training in mental pathology, and just picked up their knowledge as opportunity shuffled them along—are we quite sure that this pathology is really simple? Have enough medico-psychologically trained specialists applied themselves to the problem, and sufficiently, to maintain anything? Have we, besides, chosen the proper guides (do we even admit of such), grasped the right tools, mapped out reputable paths of procedure?

[1] "Beiträge zur Psychopathologie des Gedächtnisses," *Monat. f. Psychiat. u Neurol.*, Bd. 25, 1909; also "Beiträge zur Kenntnis der Gedächtnisstörung bei der Korsakoff'schen Psychose," *Monatschr. f. Psych. u. Neurol.*, Bd. 21, 1907; also Bd. 23, 1909.

Let me stop long enough here to dwell upon this point at issue, this point of sincerity in work and legitimacy in method, for upon it rests the future of our subject, its honor or dishonor, and whether it is to be esteemed a science, a pastime, or a dubious profession.

Reviewing this field of endeavor and its literary output, one notices at a glance that most of the energy expended has come from non-medical sources, and that it is chiefly Binet-imbued and statistically tempered. One readily notices also that neither pathology nor the individual is conspicuous in the background or visible at hand; *the test's the thing*. And the test is applied to hundreds of individuals, until the idea of test has become hypertrophied in the examiner's mind—and then we see single supposedly specific tests applied a thousand, nay even several thousand times to *test* that *TEST*, and prove it specific. In this way the child's mind at best becomes for these investigators a form into which a test must fit or misfit. The very fact that the test is the thing focused upon, and not the individual mind with its countless variations in functioning, makes the results, even where gained from ten thousand tests, diagnostically valueless.[1]

I have seen the most glaring blunders made in diagnosis, even to the extent of advising children to be sent to Randall's Island, though there was no mental defect whatever present in the case, but only a remediable retardation—because the work was done 'with a Binet' by an unskilled lay worker, and the physician in charge took the word of this worker for it. Again, of late I saw a child diagnosed as a microcephalic imbecile (and Seguin exercises prescribed for it by way of treatment) whose queer facies made me spot it outright as some atypical secretory dystrophy (*i. e.*, the trouble being due to some disturbance of internal secretion). On examination *I found its scrotum empty*, and a state of infantilism present—possibly here a case of genito-genic infantilism (no extended study could be made). This patient had been

[1] A very interesting paper that I have come upon since writing the above points this fact out very plainly—'Difficulties in the Interpretation of Mental Tests—Types and Examples,' by Frances Porter, *The Psychological Clinic*, October, 1915. See also Bobertag's comment on the test of "defining," under Comprehension.

diagnosed by a non-medical lady clinician, and all with a Binet!—although the great interest and importance of the case, as well as the urgent indication for therapy, lay in the absence of the sex glands, the mental abnormality being quite secondary. Of course, no one would deny the child's being imbecilic and probably microcephalic. But what about pathogenesis? Shall we ever get beyond the amateur stage and graduate into the scientific, if no more than this is demanded at our clinics? For is there not latent in every such (mentally abnormal) case, the possibility of a pathology which only the physician can interpret,—syphilis, ethmoid disease, tumor, hydrocephalus, serous meningitis, etc.?

On the other hand, how many physicians have really been trained in the psychological fundamentals of this work or have aided in its advance? Why does its story, in America, comparatively all come from the lay pen, either the psychologist or educationalist? Why has not the physician discovered the flaws in the work and pointed them out to the profession? Why do even these facts come from the non-medical workers?[1]

No reproach, of course, falls to the psychologists and educationalists. On the contrary, all praise must be theirs for their activities. And should our work here ever attain the stage of excellence of that abroad (at Berlin, Leipzig or Munich, for instance) the glory will be theirs, for their eyes were open and they saw, or at least tried to see, when our entire medical profession was still disinterested or asleep.

In this very city we have a hundred organizations, federations, parent associations, bureaus, etc., devoted to the study and advance of the child, mentally, physically, ethically, socially, and to the uplift of the parents in behalf of the child, with leaders who give lectures, readings, and hold conferences—all of them amateurs—and call in from time to time, or for a series of talks, other amateurs or self-assumed "authorities," or, again, some really prominent social workers, psychologists and other eminent individuals. They not only discuss the child, abnormals, remedial pedagogics, etc., but

[1] Miss Porter's article, just mentioned, is an example again.

in some circles eugenics, the sexual and the psycho-analytic as well. Besides, as already intimated, at our clinics, lay-workers have quite usurped the field.—But where is the medical college in the United States that has a *physician* devoted to the non-pediatric study of the child, the study of clinical psychology, or juvenile research (or, as it has also been termed, pædology, which embraces the many methods of therapy besides), a study scientifically pursued, with respect for precedents, a regard for authorities, an inkling to thoroughness—*based on the fundamentals of physiology, pathology, psycho-pathology, etc.?* And at what university is this subject really thought serious enough to teach, with lecture or clinic, and scientifically, on the above basis—not merely down in the catalogue! *but really taught*—and where a laboratory working in its behalf? And if the answer is *nowhere*, then this so-called Decade of the Child is, at least with us, a superficial, amateur, bungling decade, and the profession of medicine is verily losing some of its most precious heritage to the educationalist not having sense enough to come into its own.

II. Knowledge

Knowledge is made up of facts deposited in memory, deposited and retained; as they are accumulated they must be deposed and arranged *in order*, the facts being grouped and subgrouped. These groups again must be inter-linked through associations. Only in this way is knowledge reproducible, facilely reproducible. (Mere re-cognition of a thing is not knowledge unless one can associate it with some other concept, and again differentiate it from others. In a way, "knowledge is differentiation." We know a thing [a horse for instance] by knowing how it differs from other things [how it differs from a cow, zebra, etc.[1]]. This involves us in the matter of comprehension—and in truth, knowledge is based on comprehension, just as it also is on memory.) The

[1] These groups and subgroups are gathered by way of association, according to similarities; hence classes, categories, etc.—hence our word "assimilation" (ad-similare). but they are also grouped and subgrouped because of variations from such similarities; The processes of assimilating and differentiating are constantly taking place.

No.	Age.
Name.	Class.
Date. (1)	1st Examination by
Date. (2)	2d " "

1. Knowledge: (retention)

(*a*) School report: reading, writing (c. d.), arithmetic, geography, history.

(*b*) Experience, etc.:

Name, mouth, etc., *at 3*. Sex, pocket things, *at 4*.

Colors *at 5–8* [R. B. G. Y. - Br. Gr. V. P. O.].

Things in a room, on the table, goods, metals, relations, mother's home occupations, etc., *at 6*.

Money (4 to 6 coins *at 7;* all coins and bills *at 10*).

Things on the street, engines, trains, ambulance, subway, *at 6–7*.

Winter, spring, harvest, Fall, Thanksgiving, Christmas, *at 6–7*.

How does one get wood, milk, honey, ice, water, food, shoes, heat, gas, music? How is a river formed? (*at 12*).

Of what is bread, flour, butter, paper, woolen goods, silk, made?

Central Park, Bronx Zoo, Brooklyn Bridge, etc.

Days of the week, *at 9*, the months, *at 10*.

[(*c*) Words: 50 in 2′, *at 9;* 60 in 3′, *at 11;* 3 rhymes in 1′ *at 11–12*.]

2. Memory:

(*a*) I. I go to the store and buy . . . . . . (4 to 12 facts).

II. 138, 692; 5734296 III. Recalls (I).

(2 or 3 *at 3;* 3 *at 4;* 4 *at 5;* 5 to 6 *at 6–7;* 6 to 7 *at 8;* 6 to 9 *up to 12*.)

(*b*) Optic memory: I. *f.m.* II. □ *at 5;* △ *at 6;* ◇ *at 7;* *at*

(*c*) Color " I. *f.m.* II. *at 4–6*. III. naming, *at 7–*

(*d*) Repeats sentences:

(6 syl. *at 3;* 10 *at 5;* 14 to 26 *at 6;* 16 to 26 *at 7–8;* to 30 *at 9;* 26 to 40 *at 12*.)

[(*e*) Remembers from paragraph read: (2 facts *at 8;* 6 *at 9;* 7 *at 12*.)]

(*f*) Story; I. repeats II. exactness III. following visit

(*g*) Of yesterday: Last Sunday: Past: Immediate:

(*h*) Carries out commissions (3 *at 6*) . . . . . .

3. Comprehension:

(*a*) Numbers: (4 to 6 pennies *at 5;* 13 *at 6–7*, stamps 111222 *at 8;* change *at 9*)

Which is greater 6 or 10; 100 or 50; 100 or 500?

(*b*) Isolation: Complexion: Generalization:

3. COMPREHENSION:—*Continued*

(*c*) Differentiation:

Hand—foot	steps—ladder	fly—bee	at 8
Ox—horse	wood—glass	water—ice	
Butterfly—bird	paper—cloth	child—dwarf	
	Miser—one who saves		at 13–15.
	Lawyer—judge		
	King—president		

[(*d*) Fork, chair, hammer, cake, doll, cab, horse, soldier, penny, rose.]
(per usage *at 5–6;* description; per genus *at 9.*)

(*e*) Abstract ideas: ungratefulness, envy, bravery, justice, charity, *at 11–12.*
poverty, misery, evolution, revolution, advent, event, pride, *at 15–*
questions of logical causal relations, *at 13–14–*

(*f*) Ethical ideas: good, bad, wrong, sin, *at 10–11.*
Mistake-lie borrow-steal lend-give *at 12–13.*
Exaggeration, deserved and undeserved punishment, fraudulence.
What should one do if . . . .

[(*g*) Time: morning, afternoon, earlier, later, yesterday, day before, to-morrow, day after, etc., *at 6.*]

(*h*) Form: *at 6.* (*i*) Space: I. and II. *at 6–7.* III. *at 13–* .

(*j*) Directions: *at 6–*

(*k*) Weights: (arranges 2 *at 5*, 5 to 6 *at 9.*)

(*l*) Pictures: (substance stage *up to 8;* action *8 to 10;* relations *10 to 13*)
(enumerates *at 3;* describes *at 7;* interprets *at 15.*)

Physically:
Senses: S. H. T. S.
Speech:
Sensory-*motor:* (agility, dexterity, etc.)

Diagnosis (clinical):
" (psychological):
Mentally:

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4. COMBINATION:

(*a*) Orientation: (time, place)

(*b*) Geographical sense: (how get to . . . . . . from . . . . . .)

(*c*) Arithmetical problems (see 1 (*a*)).
$5 + 6 =$ $32 - 11 =$ $18 \div X = 6, X =$
3 loaves among 2 boys? Months in $\frac{3}{4}$ year?
If 3 eggs cost 20 . . . . . . I am thinking of a number.
If I add 5 I get 12 . . . . . .

(*d*) Completion tests:
I. Ship-storm-lost. New York-money-river.
(Mountain-lumber-wealth.)
(in 2 sentences *at 10;* in one sentence *at 11–*)
II. Sky-red. Sun-midday. Snow-muddy.
(Drinking-poverty.)

4. COMBINATION:—*Continued*

III. Put dissected sentences together (B's 1, 2, 3) *at 11-*

IV. Though it is raining . . . . Even though the soup was burnt . . . . Even though I am ill . . . .

V. Compose a story out of: winter-night, soldier, bitter-cold, froze, relieved, Death, *at 13*.

(*e*) Part-picture test: I. (animals) II. (blocks) III. (B's) *at 7*.

(*f*) Many cooks. . . . People who live. . . . He who digs. . . . Changes mind. . . .

(*g*) Grasping the "point" of a story *at 7*. I. II. (4. (*d*) V.) *at 15-* III. Interprets picture (3. (*l*)).

(*h*) Critique: I. Exaggeration II. Absurdity (B's 5) *at 11-* III. Of personal acts.

(*i*) Problems in judgment, etc. I. B's 1st series *at-10*. II. 2d series *at 11-12-* III. Cutting paper *at 13-* IV. Reversed triangle *at 13-* V. De Sanctis' No. 6 (*a*): (*b*,) *adult:*

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5. ATTENTION:

(*a*) Scope: I. Observation in room: II. In picture: III. Exposure test:

(*b*) Concentration: I. Reversed association (7 2 9 *at 6;* 20-1 *at 8*). days, months, name. II. Simultaneous addition: III. Counting syllables:

(*c*) Tenacity: I. Cancellation: II. Assortment: III. Dots:

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6. FEELING:

(*a*) What good deed have you ever done?

(*b*) A sparrow had a nest. . . .

(*c*) Example of want (with theft), cruelty, joy, etc. (note comment).

(*d*) Aesthetic choosing - 1, 2, 3, (of Binet's) *at 6*.

(*e*) Aesthetic judgment of pictures *at 10-12-*

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7. RELIABILITY OF MEMORY: (suggestibility, etc.)

(*a*) Exposure test:

(*b*) I. Picture II. (3. (*l*)) III. Recognition test:

(*c*) Auditory (news account):

[These sheets may be obtained from the Fetzer Press, 62 Reade Street, New York.]

greater the number of associations the more readily the ability to get hold of the desired fact—and the more apparent the knowledge.

Our accumulations of facts are multitudinous. To test such inventory, therefore, one could not catalog it all. One can, however, test important deposits here and there, and thus roughly judge the nature of the whole, for our training and education are in a way much the same, and the com-

munity to which we react is also for most of us the same. When this is not the case we may have to examine very differently and make ample allowances. The child from the country, for instance, has gathered different knowledge from that reared in the city. Still greater is the difference when the child comes from some foreign land.

In testing this *Knowledge*, we must bear in mind that we stand on rather problematical ground, and must know how to be both discerning and charitable. There is school knowledge, attained at school, and that gained through experience (picked up at home, on the street, from others, etc.). A child, after all, can only know that which it has been 'exposed to,' so to speak. As it grows older, it does not by any means retain all it has gathered. By the time it gets to adult life it has long since forgotten an astonishing lot.[1] If the 'educated' were tested, writes Scholtz,[2] much shocking ignorance would come to light, for, indeed, we retain only that which we use in our daily life, that which we hold through much repetition, and that in which we are especially interested. We will be careful, therefore, and especially in adults, to make no final or indelible appraisement until we have carefully considered the facts, weighed all 'extenuating circumstances,' and, when necessary, made liberal concessions.

The Tests for Knowledge

Our procedure is as follows: first of all (*a*) we note the school report (obtained from the teacher) of the child's standing in reading, writing (from copy and from dictation), arithmetic, geography, and history, for the teacher who sees

[1] See Rodenwald's study of 174 recruits. *Monatsch. f. Psychiat. und Neurolog.*, Bd. 17, 1905. He found that there were hundreds of facts you would expect a man to remember that had long since slipped out of his mind. Even the simplest questions were at times misanswered.

Similar remarkable results were obtained in testing lower Italian Folk-classes. The tests showed that these people live apart from all such world interests as politics, religion, culture, etc. See Pasta Lombroso-Carrara and Mario-Carrara, Turin, 'Aus der Vorstellungswelt des niederen Volkes,' *Morgen* (*Wochenschr. f. deut. Kultur*), No. 13, 1908.

[2] L. Scholtz, 'Anomale Kinder,' Berlin, Karger, 1912, p. 102–3.

the child day in and day out gets a very fair idea of its various abilities. (We may use the letters E [excellent], G [good], F [fair], P [poor], and X [failure], in checking the facts.[1])

Under (*b*) we probe knowledge gained through experience, and must needs give a considerable number of questions to determine this. No doubt much of the knowledge thus brought out is taught at school, or at least touched upon there. However, there is no necessity of indicating any sharp dividing line.

We begin by asking the child for its family name, then have it touch its mouth, ear, eyes, nose, etc. (in Binet's way), and ask whether it is a boy or a girl; we also have it recognize pocket things, namely, handkerchief, key, knife, penny.[2]

The number in italics, to the right of the tests, gives the approximate age at which such test should be passed (*i. e.*, the age at which these facts should be known or answered). *Where a hyphen follows such number, it indicates that the question may be normal even at a higher age.*

Concerning *colors*, much divergence of opinion exists among authorities as to when they are normally learned. Some give the age as early as four, others as late as eight. Red, blue, green and yellow are first learned, and later brown, gray, violet and orange. According to Ziehen[3] (and Meumann) the normal child knows the chief colors, inclusive

[1] It will be well to compare the school opinion and standing with our own results at the end of the tests. It may be remembered, however, as Stern points out, that the school work (and marks and advancement) depend not only on intelligence, but on facts that have nothing at all to do with intelligence, but belong to the domain of will; *i. e.*, intensity and perseverance of attention, diligence, conscientiousness, sense of duty, etc. Stern believes that, because of these facts, the intelligence examinations have shown a very large number of children to be pedagogically graded below their 'intelligence age' (according to Binet). See W. Stern, 'Die psychol. Methoden d. Intelligenzprüfung,' etc., Leipzig, 1912, p. 39, et seq. Stern also points out (p. 79) that spontaneous intelligence cannot be gauged by tests, and is therefore excluded. So we must seek to estimate it through a criterion that lies outside the experiment. As such the teacher's appraisement of the pupil offers itself.

[2] Terman and Childs place the knowledge of sex and pocket things in the third year. For the knowledge of the normal child up to its third year (from the first to the 1000th day), see W. Preyer, 'Die Seele des Kindes,' Leipzig, 1905 (6th edition), p. 399.

[3] Ziehen, 'Die Erkennung des Schwachsinns im Kindesalter,' Berlin, 1909, p. 24.

of black and white, at the end of its third year. But green and blue, brown and gray, may often yet be confused by the normal child in the fourth year, exceptionally even in the fifth. In the feeble-minded knowledge of colors comes much later, and may be defective, while the imbecile may never gain a conception of colors, or only the most primitive. Gaupp[1] gives the average age for color-knowledge at six. A study of Warburg's[2] testing 1,800 children showed that color *naming* might be used, in a measure, as an intelligence test, inasmuch as the lack of color naming in this series of tests went parallel with intellectual defect. Binet's test of the four fundamental colors in the seventh year, is of this type. Bobertag found that only a half of the seven year olds can pass the test. (But see under memory.)

In testing '*things in a room*,' I show a picture of a room containing chairs, table, mantel, lamp, wall-pictures, etc., and ask: "What things do you see in this picture?" One may also ask the child to point out a chair, picture, etc., in the room it is in. A child finds this much easier than *naming* a thing you point to (in a picture or a room). *In all naming tests more mistakes are made than in indicating tests.* Ziehen remarks (l. c., p. 57) that the word may be missing, although the idea (Sachvorstellung) be present and understood. '*Things on a table*' are knives, forks, spoons, etc. By 'relations,' sisters, brothers, aunts, uncles, and cousins are meant.

As to money, much again depends upon whether or not the child comes or has come in contact with it. In eliciting this knowledge, show a cent, nickel, dime, quarter, half-dollar, etc.

[1] R. Gaupp, 'Psychologie des Kindes,' Teubner, Leipzig, 1910, p. 59. Much depends, he says, upon whether colors are shown and the indicating of colors (Farbenbezeichnen) practiced. How much in fact depends upon the constant *re-seeing* of colors and hearing the names of such, is shown in the case of a child who at seven lost her eyesight, regaining it again at 17. In the meantime she forgot all her color knowledge, and at 17 had to re-learn almost as she did in the beginning. O. Heyfelder, 'Die Kindheit des Menschen,' Erlangen, 1858, second edit., p. 13. O. Bobertag points out that there is a very great difference in the age of color knowledge according to whether a child is of the lower or higher classes, also between males and females, the latter being more proficient. 'Über Intelligenzprüfungen nach der Methode von Binet u. Simon,' Leipzig, 1914, p. 89.

[2] *Münch. med. Woch.*, No. 49, 1909.

By '*things on the street*,' horse, wagon, car, policeman, letter-carrier, soldier, etc., are meant.

The following facts (see next line on intelligence sheet) may also be brought out by inquiring "When does one use sleds?" "When does the snow fall?" "When does the grass come up?" "When do the leaves fall (or turn red)?" "When does one gather in the hay, fruit, crops?" etc.[1]

The succeeding lines contain questions of varying difficulty, and no note of age is made, it being as yet undecided when a child ought to have this knowledge. One may ask older children or adults still more difficult questions, *i. e.*, "Of what are houses made?" "What is the horizon?" "What is a shadow?" The meaning of "oriental," "civilized," etc., ('taxes,' 'insurance,' 'jury duty,' 'preparedness'). Often enough there is no exact, not even approximate age at which one can say these facts become known to a child (reading, school instruction, etc., having much to do with it). One learns considerable, however, of the mental ability, from the answers given. One should constantly bear in mind that lack of knowledge does not always mean mental inability. Ask adults, for instance, "Where does bronze come from?" "What is soap made of?" "Cheese?" "Linen?" "What's a tide?" "Light?" "The Aurora?" "An atom?" etc.[2]

We next have the child tell about Central Park, or the Bronx Zoo, etc., and note the way it describes what it has seen, what *animals* it knows, etc. I also inquire how it gets there, to learn its sense of orientation—which fact may then be noted under 4 (*a*).[3]

[1] The question, "What is autumn?" sometimes proves a sticker. "Thanksgiving" may be too difficult for children not learning of it in school. One of my students first found it correctly answered (tested on orphan asylum children) at nine. One may also ask "Why do we celebrate the fourth of July?" How bread is made should be known, according to Cimbal, at 10.

[2] And both Rodenwald and Ziehen (l. c.) point out that normal adults may even go wrong on questions like "On what river does the city lie in which you live?" "Who was the last mayor, governor, etc.?" "Who is in office now?" *Especially if frightened*, an individual will go wrong on questions like this. *If the individual being tested is frightened, you can place no value whatsoever on your examinations if the results prove negative.* This is an urgent lesson for the courts to learn.

[3] Under the tests for combination ability.

One finally inquires as to the names of the days of the week,[1] and of the months (the former, according to Binet, known at nine, the latter at ten). The date, as to the month and year, is also known at nine, but Scholz warns against asking for the date of the day, as adults often cannot answer this.

As to (*c*) *words*[2] (and language), one must also here know how to make allowances. Some children are rich in words through natural talent, some through much reading, some through a good home, a governess, etc. Children rich in language 'pass off' splendidly; yet such children are not necessarily superior in intellect to those who have a poor verbal store. Mehnert at the Dresden Congress[3] alluded to two children he had seen at two different schools (of different social standing), the first being brought to school by its governess, the second coming alone and with the house key tied about its neck. And he commented: "How many more practical duties that latter head fulfills and is capable of than the child of the richer class, who in the simplest practical urgencies may stand helpless. Must we not take this into account in the mental evaluation?"

There are most capable laboratory workers who conversationally are startlingly uncouth. Again, there are artists whose language ability is of the most primitive type, and who could not for their lives tell a connected story, and yet, at their canvas, with crayon or brush, their fantasy veritably becomes creative and alive. Dohrn[4] intimated

[1] In order to be sure the child does not spin these off automatically, have it begin with "Wednesday" and so on to "Tuesday." Also ask, what day was the day before yesterday; what day will be tomorrow.

In Bobertag's tests, 75 per cent. of the 8-year-olds knew the days of the week; hence, he believes the test ought to be placed in the eighth year. Asking for the date, however, he does not approve as a suitable test, and the naming of the months is likewise questionable (depending on practice).

[2] Bobertag (*l. c.*) points out that this test of giving 60 words in three minutes is found decidedly unpleasant even by adults, and that really intelligent children may fail on it. He thinks it of no use as an intelligence test. A much greater number of words can be found by the examinee if you lead him, directing what groups of words he should choose; thus 'things indoors, outdoors, flowers, animals,' etc.

[3] Deutscher Kongress für Jugendbildung und Jugendkunde, Dresden, Oct., 1911. Report pub. by Teubner, Leipzig, 1912.

[4] At the Dresden Congress just mentioned.

that there may be children of this same type, poor in words, in language, but eloquent enough could they express themselves with line or color. *This, too, is intelligence.*[1] We might also recall to mind Oliver Goldsmith, 'who,' as Garrick penned, 'wrote like an angel, but talked like poor Poll.'[2] There are many children, and adults too, who, like him, are too shy, or too 'inhibited,' or too nervous to speak—who can never speak well, especially not on 'occasions' (for just then they get particularly nervous or panicky)—and yet can *write*, even with exceptional skill. Again, some 'temperaments' are even brilliant in certain surroundings and almost mute in others, or, as Washington Irving tells the story somewhere, one day a nonentity at the foot of the table and on the next at its head, and its very life.

On the other hand, there are children (and adults) very glib of speech, ever ready with an answer, with so many answers, that conversationally they appear most intelligent, but who, on careful scrutiny, show themselves very superficial, who never have their ideas sharply focused, and whose answers, as Scholz puts it, never taper to accuracy. These are the l'esprit d' à-peu-près of the French.[3] In the adult this type must be distinguished from the neurotic individual who has an impellence to speech and little inhibition to stop, and whose restless and awake nature causes him to read much and be informed on every *new* or scientific subject (most superficially, of course—or not informed at all!). In the hysterical the same thing occurs, only these individuals also wish to be in the lime-light, wish to be admired and

[1] Not long ago I heard an artisan cross-examined about the mental ability of one of his children—a child evidently not of the resplendent kind—say, "Everybody can't be a school teacher, everybody isn't smart that way; but one can be smart with the hands too. My one boy is a mechanic, and very good, and the girl was two years at jewelry work. Taking proper care of a home and doing everything in it is also smart, isn't it?" And this is true. Who knows if the schooling of such children (his children, through change of domicile, had gone from school to school, and were part of the time in some charitable institution) had been ample and continuous, they might not have been quite up to the shining mark of other children of that age?

[2] The estimate formed of young Goldsmith by his contemporaries at school was that he was a dull boy, 'a stupid, heavy blockhead!' (William Black in 'English Men of Letters Series').

[3] See Scholz's excellent description of this type; 'Anomale Kinder,' *l. c.*, p. 108–9.

esteemed, and will argue on every topic under the sun, and never give in. They are terrible individuals to have to meet socially, and leave alive!

As to the test of *rhyming*, the last in this rubric, it is not an important one, for rhyming may be a matter of special talent and not of intelligence. Besides, the nervous child may at times be unable to hit upon a single one in your presence, yet at home can find a dozen. Bobertag noticed that some children could not quite grasp what was wanted of them, and again, others, who understood and were intelligent enough, could not come upon the word. One would scarcely say, he comments, that this failure indicates a lack of intelligence.

III. Memory

Before taking up the tests of memory, certain psychological data may be advantageously reviewed for the sake of brief orientation and for the better understanding of the irregularities we are likely to find in both our normal and abnormal charges.

In the process of perception we assume[1] a physiological activity or change in the cortical cells stimulated, leaving its trace in a nigh permanent alteration, either in the disposition of the molecules or through a discharge of their ions, or a metabolic generation of a kind of *residuum*. These cells thereafter are predisposed to the same excitation, and should such again come to them, the former presentation again becomes conscious. This alteration, this *residual something* is memory.

Yet not singly but in complicated clusters are these cells originally stimulated, depending upon the various and manifold perceptions occurring at the same time; and ever after, when any one set of cells is innervated, a hundred others will be consorted and set vibrating and many series of memories become conscious. Hence, our association of ideas.

Here, then, we see systems of memories grouped and relayed, not in near-lying bundles of cells, but over large areas—

[1] Following the teachings of Ziehen. See his 'Das Gedächtnis,' Berlin, Verlag Hirschwald, 1908.

circuits, we might think them, connecting with other circuits. The same cells or groups may be used in several or even in many circuits, so that from such 'cell station' one could start off on different association tours (from the association 'John,' for instance, we might start off on 'John Brown,' or 'John Bunyan' or 'King John' or Shakespeare's play, or maybe a friend of that name; or, through a side relay, from the name 'John-a-Dreams,' to poetic imaginative characters, or to etymology, etc.).[1]

And how actually real these large circuits are, may be gathered from the common experience that if we do not take trips over them once in a while the trail becomes obscure—may even be lost. More remarkable is the fact that in certain conditions, as hysteria (one might say the make-up of certain individuals is prone to this), whole circuits of associations are temporarily switched out at times, dissociated from these minds, so, for instance, all the memories associated with a certain event, or a certain bodily function, or the entire knowledge of a language, the mother tongue being retained. Again, through disease, circuits covering certain periods of time are sundered—there occurs an amnesia—sometimes, but transiently, to return, sometimes gone for good. Finally, through destruction of brain tissue (hemorrhage), many wires, or whole stations, may be destroyed, and great accumulations of associations blotted out (aphasia).[2] In childhood, disease processes (inflammation) may also damage the cells and connecting tracts, which never regenerate; or individuals are born with a defective brain, the cells being spoiled from the start.

There is still a remarkable fact to be noticed in the

[1] Just how we happen to go off on one or other of these associations depends upon how we are *constellated* at the time. For the psychology of this see Ziehen's 'Leitfaden d. Phys. Psychol.,' 7th edit., p. 186.

[2] When such aphasias clear up in part (as they usually do) this may occur through the absorption of blood or clots having caused pressure, or the regeneration of such cells not too severely damaged, or through other cells and fibers taking over part of the work. Again, the local destruction of nerve fibers may cause distant cells or groups to degenerate. See Monakow's *Diaschisis theory* ('Ueber Lokalisation der Hirnfunctionen,' Bergmann, Wiesbaden, 1910, p. 19). See also Monakow's larger work 'Die Lokalisation in Grosshirn,' etc., same publisher.

mechanism of memory. We spoke just now of the cells, when stimulated, becoming conscious and undergoing a change in which a latent something is left behind, so that if similarly stimulated at a later time, such cells re-awaken their former 'content'—awaken to consciousness again. This statement we must, however, modify. There are some cells which subsequently lose this quality of consciousness, and functionate without awareness. This functioning, now (we say) becomes *automatic*. Thus the feat of walking, which the child first learns so laboriously and consciously, becomes automatic; one acquires a language, and consciously works with rules of grammar, regulations, etc., and yet later speaks fluently, without a thought of the words or rules themselves—speech coming automatically. Even our first speaking, writing and reading are all learned consciously, and with heroic exertion and effort, later to become automatic. The same is true of piano-playing, bowing and fingering the violin, typewriting, dressing, eating, shaving, riding a wheel, etc.[1] We can continue these processes agoing, and have our conscious minds active in quite another direction (thus a man may walk, hum to himself and whittle a stick quite automatically, pursuing some train of thought at the same time). Automatically certain words in our process of thinking follow others (as 'either-or,' etc.)[2] and likewise one act may follow another (as when we are about to sneeze and almost at the same moment grasp for our handkerchief—sometimes even heave up with a 'thank you' before the expected 'God bless you' is given. In this latter instance we have an automatic ideational association, not only an automatic act. Such have been termed 'psycho-reflexes' (Bechterew), and quite a psychology has grown about the problem.[3] Even

[1] The opposite process has also been noted: the infant at first suckles through instinct (*i. e.*, a phylogenetic automatic act); but little by little this becomes a conscious act. See Compayré, 'L'Evolution intellectuelle et morale de l'enfant,' p. 175.

[2] And such words automatically influence and lead our thinking just as do analogies, contrasts, etc., and cause whole troops of ideas to be marshalled in one or other direction.

[3] W. v. Bechterew, 'Was ist Psychoreflexologie?' *Deutsch. Med. Woch.*, No. 32, 1912; 'Ueber die Entwicklung der psychischen Tätigkeit,' *Deut. Med. Woch.*, No. 47 and 48, 1913; *Zeit. f. Psychother. u. med. Psychol.*, Bd. V., H. II; 'Ueber die Motor-

logical conclusions and judgment may be consummated automatically, whole series of links being skipped. Our popular term for it is 'jumping at conclusions.'[1]

What has happened here? It is possible (as animal experiment has intimated)[2] that the optic thalamus takes over some of these activities, and 'runs off' sets of associations automatically, with or without the cortex participating (for in all these acts the cortex can be made to participate, the automatic act be made conscious; there is even a frequent alternating of the voluntary and automatic). At any rate, much that we have made tracks for in our brains—possibly far more than we at the moment are aware of (Bechterew's studies seem to indicate this)—once started, tends to go off automatically. Thus the recalling of addresses, fore and surnames, telephone numbers, multiplication tables, much that we 'ground' at school, old pieces, jingles, poems, music that we 'know by heart,' tunes that 'run in our heads' and songs generally, and all that we 'commit to memory.'[3] There is an unsolved mystery latent here which has not been helped a whit by the metaphysical adaptation of an artificial subconscious, or by assuming we have different kinds of memory, a rote or automatic memory, a voluntary memory, etc., for, as we have seen, they are to a certain degree interchangeable, and one may pass into the other. But we must keep this automatism and its possibilities[4] in mind, and in

ischen Assoziationsreflexe,' R. Golant, *Ergeb. d. Neurol. u. Psych.*, Bd. I., H. 3, 1912; W. v. Bechterew, 'Objektive Psychologie od. Psychoreflexologie, Die Lehre v. d. Assoziationsreflexen,' 1913, etc.

[1] See Ziehen's 'Leitfaden,' etc., *l. c.*, p. 197; E. Meumann's 'Vorlesungen,' etc., *l. c.*, Vol. I., p. 539; Störring's 'Experimentelle Untersuchungen über einfache Schlussprozesse,' *Archiv. f. d. ges. Psychol.*, XI., 1908. Such skipping of links in the association of ideas may become pathologically exaggerated, in which case we have the condition of "incoherence." What is known as stereotypy may also be thought of in connection with this mental mechanism (of automatism).

[2] Ziehen, 'Leitfaden,' *l. c.*, Chapter II. Another theory is that of Grasset's. See his 'L'hypnotisme et la Suggestion,' Paris, 1903.

[3] In fact much, if not most, of what we have automatically in mind, cannot be given consciously. To get your telephone number you 'start it' consciously, and then run it off automatically. To note the difference between this and the conscious effort, try to give the number backwards!

[4] How amazing and involved this automatism can become is seen not only in lightning calculators who show phenomenal feats of arithmetical combining, etc., but

our testing remember that some answers come mechanically, others again with conscious effort, and that some subjects, like arithmetic, may be handled by certain minds quite automatically, occasionally in the most astounding fashion. Much too, if not the most, that imbeciles 'know' is remembered automatically.

A few additional facts should also be borne in mind. Somehow, in the first two, or possibly three years, the residuum does not hold—it is almost as rapidly washed away as formed. That is, probably, why we remember nothing of this period.[1] But in the next few years what is gathered in mind is almost indelibly held there (which shows the urgent importance of taking care in these years what is fed to the child's senses, to store up in mind). There is something materialistic about it all, something more physiological than psychological; and hence, according to the kinds of mental (physiological?) "material," and, as such varies in different individuals, we have many types and variations and capabilities of memories. Yet, in the process of development, all memory capabilities are not given at once. The child of six has capabilities that the child of four has not yet acquired. As the brain matures the capabilities expand. Factors of perception and comprehension play a decided part —*form* is remembered much earlier than *time* (because comprehended much earlier), certain colors much earlier than others (the retinal rods and cones which take up certain colors presumably developing later than those which take up others), etc. Finally, when memory begins to dwindle because of disease, it goes in the reverse order that it has been acquired, and the childhood's quantum is held to the very end.

in the cases of epileptic automatic acts, and especially the reported cases of somnambulism. A drug clerk may under these conditions compound different prescriptions, point out incompatibilities, and refuse to give poisons, etc.; or another individual compose verses, or solve problems, etc., all automatically in sleep. See Loewenfeld's 'Somnambulismus und Spiritismus,' Wiesbaden, 1907. For everyday examples see Dessoir's 'Das Doppel-Ich,' Leipzig, 1896, p. 9–11; for examples in hysteria, hypnosis, etc. see Janet's 'L'automatisme psychologique,' Paris, 1889.

[1] See Compayré, *l. c.*, Ch. 6, Pt. I.

The Tests for Memory

Under (*a*) one plays make-believe, and sends the child to the store to buy several things: a loaf of bread for five cents, a bottle of milk for eight, a half pound of butter for ten or fifteen cents (and possibly adds a pickle or something for the dog). After telling the child to remember these facts, one gives it a series of numbers to repeat. Two or three series are given. In the third line will be found the number of figures a child is supposed to remember at the various ages: two or three at the age of three, for instance, three at the age of four, etc.[1] (In the space at the right of the tests, and corresponding to the second line, one notes with E, G, F, P and X, accordant with excellent, good, fair, poor and failure, how the child has passed this test. Or one may merely use a check (√) to note that the test is passed). One finally inquires of the child what it purchased at the store. To reproduce this, the child will have had to carry it in mind—to have retained or remembered it. (One again marks this result in the space to the right.)[2]

Under (*b*), optic memory, there are two tests: (I), one first puts questions as to the form of certain things, '*from memory*' (the full moon, young moon, a wheel, stamp, chimney, [pyramid] obelisk, etc.), then, (II), has the child copy certain figures, which are exposed a brief time (15′) and again covered over (for 15′). For this purpose one uses a square, a triangle, a diamond, and a more complicated figure (such as Ziehen's).

In the test of copying the square and diamond *placed*

[1] As to remembering numbers, Ziehen points out ('Intelligenzprüfung,' p. 18) that even the paretic and senile dement, and even the severest *Korsakoff* patient will carry three numbers and first stumble on the fourth. However, even Korsakoff patients may be able to retain six numbers, exceptionally even seven. But they will long since have forgotten the example under (*a*). An adult, therefore, who cannot repeat 3 or 4 numbers shows grave defect. Under such conditions simulation must also always be borne in mind. The retaining of numbers is, in the case of most people, a matter of *acoustic* memory. Bobertag (l. c., p. 59) holds this repetition of a sequence of numbers as more really a test of attention ability. Meumann appears to concur in this (*l. c.*, Vol. II., p. 260). Test (*a*) may also be varied thus: I. Give six words to hold in mind. II. Give two short mental examples. III. Now ask for the six words.

[2] The method of marking and grading is taken up below.

before it, Binet[1] found that this was accomplished at five and seven respectively; the triangle, according to Saffiotti[2] at 6. Here the test was not a memory but a talent test, the drawing remaining before the child. But Meumann appears to have employed the test (and at the same age) 'mit Abdecken,' *i. e.*, by covering the drawings in part, or several times. So, in using this as a memory test, I have left the age level the same, though probably this will have to be modified, placed somewhat higher.

Ziehen's Test-Figure.

Color memory, (*c*), is tested (I) '*from memory*': Color of a one and two cent stamp,[3] strawberry, lemon, grass, sky, potato, coal, corn-flower, leaf, moon, warship, American flag, etc. For the second test, (II), *recognizing* colors, one shows several colored skeins of yarn, or strips of paper (for ten seconds), and then covers them over; after a few minutes a larger assortment is placed before the child, who is asked to

[1] One must not *tell* the child to 'draw a square'—one *shows* the square (1 to 2 inches in size) and then instructs the child to copy it. Binet gives pen and ink, this being more difficult than drawing with lead pencil. In drawing the diamond one half of his children failed at six, even one-fifth failed at seven. It would be interesting to know if any real differences occur, the child using pencil, pen or brush and paint.

Bobertag's statistics (testing with the diamond) are: at the age of six, 10 passed out of 31 (32 per cent.); at the age of seven, 27 out of 44 (61 per cent.). He also asks the child what it thinks of its own drawing. This, however, is *criticism*, and will be dealt with under 'Combination.'

[2] Treves ed Saffiotti, 'La scala metrica dell' intelligenza di Binet e Simon,' etc., Milan, 1911.

[3] Ziehen points out that mistakes between green and blue in the matter of stamps 'from memory,' are numerous in normal persons.

pick out the colors already shown. (Should be done at four to six. See also under Knowledge.) The *matching* of colors may also be employed in the same way. Test III. is of *naming* colors (cf. Warburg, *l. c.*). Binet shows four squares of colored paper (red, yellow, green, blue) pasted on a cardboard and has the child name them—"the verbalization of the colors perceived." He asks "What color is this?" pointing to one after the other. No errors whatsoever may be made, no reconsidering, etc. This must be passed at seven. Bobertag, as already stated, found that but one half his children at seven passed and therefore puts the test in the eighth year.[1]

Under (*d*), sentence repetition, any simple sentences may be given. (Binet's do not appear to be very well chosen.) One may make up sentences, giving several and increasing the length, having the child repeat these. One then knows about how long a line a child easily holds, and counts the syllables. Or one may write out a series of sentences in which the number of syllables is known, and use them as constant tests. Just below this test one will again notice, in brackets, how many syllables a child is supposed to remember at a given age. These sentences are to be repeated without an error, no word to be omitted, nor other substituted. The following are examples of sentences: "My brother went away" (6 syllables); "We have not finished our lessons" (8 syllables); "I am going to my mother to-day" (10 syllables); "Let us all go for a long walk this afternoon" (12 syllables—suggested by Bobertag, and considerably better than Binet's[2]). The last sentence may be lengthened to 16 syllables, thus: "Let us all go for a real long walk through Central Park to-morrow," etc.

[1] Meumann, on the other hand, asks for the naming of six fundamental colors (Hauptfarben) in the fifth year under his environment-tests; in the seventh year, for seven chief colors and three nuances (also the colors of the 5, 10, 20, and 40 pf. stamps, and the states' and country's colors); in the eighth year, for ten colors, also tests with exposing and covering of colors, and naming from memory similarly colored things. See his 'Vorlesungen,' etc., schema, at end of Vol. II.

[2] "I am cold and hungry" (6 syllables); "My name is Gaston—Oh, the naughty dog" (10 syllables); "Let us go for a long walk—Give me the pretty little bonnet" (16 syllables—Binet's—from Miss Towne's translation).

Under (*e*) Binet's paragraph may be used, or Goddard's. In this test the child reads the paragraph aloud. *It is a questionable test*, much depending upon the child's ability to read, whether its attention holds, or is distracted, how dramatic the paragraph is, etc. Its evaluation, finally, is extremely difficult.

Under (*f*), story remembrance, a short story, fable[1] or definition may be given. I have found the following often useful. I give the child a definition of an optimist (something that it probably never heard before): "An optimist," I say, "is a person who is always happy, always cheerful, always smiling, even when it rains or things go wrong. He always sees the bright side of everything." I have the child (*i*) repeat what I have said, noting (*ii*) the exactness of reproduction, and see to it that he has understood. I also have him promise to be an optimist. (*iii*) The question is again put on the following visit. Such a definition (because of the abstract involved),[2] as also the Binet and Goddard paragraph (because of the many incidental facts given), are much more difficult to repeat and remember than a fable or complete story. A good story, however, is used in the tests under *combination* further on, and it is well to compare the outcome of this test with that under combination.

Under (*g*) we test very recent, recent and more remote memory. We have the child tell what it did yesterday throughout the day, or what it had for dinner,[3] also what it did last Sunday. We likewise ask where it lived last year, went to school, etc. (Or one may ask "Tell me what you have been doing and where you have been in the last two years?") In adults this test is often of great importance, and the questions must be extended, and immediate memory also tested, *i. e.*, "Where were you just before coming here?"; "How did you come in?"; "What question did I just ask

[1] Terman and Childs give a fable, and find that it is repeated at ten. Meumann also uses this (fable) test.

[2] Here memory alone, and not comprehension, is tested. However, memory is always much aided through comprehension.

[3] But it should be remembered that even normal adults very frequently do not remember this.

you?" etc. We can also enquire about certain books read, or some special book, and what is remembered of the story. Or we learn of certain happenings in the family, and question the patient concerning them.

The carrying out of commissions (*h*) is Binet's test (puts keys on chair; shuts door; brings box). Bobertag holds this as almost wholly a memory test, though attention plays a part, and likewise thinks it somewhat too easy for the sixth year.

When doubtful as to the results of our Memory examination, or if a fuller examination is desired, other tests, such as Ziehen's *pair-words* method may also be tried; *i. e.*, one gives ten pairs of words, each pair composed of a noun and an adjective. After a period one gives the noun of each group, and asks for the adjective. (See *Monat. f. Psych. u. Neurol.*, Bd. 9, 1901; also 'Psychiatrie,' 3d edit., p. 229.) Ranschburg uses a similar method, giving street names and numbers. For other methods of examination, especially with apparatus, see second half of P. Ranschburg's 'Das kranke Gedächtniss,' Leipzig, 1911. On memory examination, etc., in pathological states, see A. Gregor's 'Leitfaden der Experimentellen Psychopathologie,' Berlin, 1910, 8th lecture. See also M. Offner's 'Das Gedächtniss,' Berlin, 1911, especially Chapters VIII. and IX.

Let us pause here to take a look at the *bookkeeping* entailed so far:

2. MEMORY:	normal
(a) I. I go to the store and buy......(4 to 12 facts).	E. very good
II. 138, ✓692; 573✓296 ⓟ III. Recalls (I). ✓	
(2 or 3 *at 3;* 3 *at 4;* 4 *at 5;* 5 to 6 *at 6–7;* 6 to 7 *at 8;* 6 to 9 *up to 12.*)	
(b) Optic memory: I. *f.m.* ✓ II. ✓ *at 5;* *at 6;* *at 7;* *at*	due to poor vision ?
(c) Color " I. *f.m.* ✓ II. *at* 6. III. naming, *at 7–*	
(d) Repeats sentences: ✓ 30 syl.	g
(6 syl. *at 3;* 10 *at 5:* 14 to 26 *at 6:* 16 to 26 *at 7–8;* to 30 *at 9;* 26 to 40 *at 12.*)	
[(e) Remembers from paragraph read: (2 facts *at 8;* 6 *at 9;* 7 *at 12.*)] 8 facts.	E
(f) Story: I. repeats ✓ II. exactness F III. following visit g.	g
(g) Of yesterday: ✓ Last Sunday: ✓ Past: ✓ Immediate: ✓	✓
(h) Carries out commissions (3 *at 6*)......6	E

The case from which the above is taken was a child of ten years with corneal opacities, the right eye having such opacity near the center. The clinical diagnosis was that of congenital syphilis, with a question mark. Physically the child was normal; the senses were also normal, save for the lack of vision in the right eye. This child was almost in

the proper grade for her age. The memory examination here shows remembering and recalling to be excellent (the '7' inside the circle signifies seven digits repeated): recalling of forms (from memory) normal; but copying of the diamond was but fairly, if not rather poorly, done. This copying went slowly, and was accomplished with much effort. The first color test was normally passed, the second very poorly. It was evident, from the way the girl went about it, that her poor vision accounted for this. Hence, in the margin, we noted the fact. The sentence repetition (test *d*) was good, the remaining tests (save the second, under *f*) were good or excellent. Hence, in the right-hand margin we wrote at the top "Normal memory," also the comment under this "Very good." Here, at a glance, we can see what was good, and what was not good in the child's memory. The note as to the defective vision explains the poorness of some of these tests.

All in all, our memory tests embrace both the ability to take up or 'take in' new impressions (*i. e.*, momentary *impressibility* of the mind for new matter—the German Merkfähigkeit, corresponding to our 'rote memory'), as well as the faculty to retain the matter absorbed. Children are astonishingly capable in both—hence meager capability makes us suspect a defective mind.

There is great variability, however, in normal individuals as to their endowment in these faculties. Some take up new ideas rapidly, but forget them rapidly. Others learn slowly, or with difficulty, but the matter 'sticks' and is remembered. Others again, even though they learn with difficulty, nevertheless forget easily.

The taking up of ideas is influenced greatly by attention and interest. When we tire attention fags, and things taken in won't 'stick'—the impression doesn't hold. Hence, where there is tiredness and fatigue (when the brain is anæmic the same condition obtains) or lack of attention or inability to attend (because of lack of interest, poor teacher, or pain, hunger, etc.) things are not learned, *i. e.*, are not taken up nor retained. This is very important to remember. But still more important is the fact that the abnormal child, especially the debile or light imbecile, has a greater fatigue-ability than the normal, and that the results of an examination may be very different according to whether the child is fresh and rested, or physically and mentally tired.[1]

Another point of interest (possibly of great importance) is that some studies have shown that during periods of rapid growth the intellectual functions are weaker, that the energy for growth is drawn upon at the expense of the energy used in the intellectual development.[2] This probably involves all the intellectual faculties, not

[1] O. Binswange rhas called especial attention to this. See 'Jahreskurse f. Aerztl. Fortbildung,' 1912 (3 Jahrgang).

[2] See Claparède's 'Studies in Child Psychology and Experimental Pedagogy,' Chapter 4, No. 2, and other literature cited by him.

memory alone. The ages between six and seven, and fourteen and fifteen are the periods especially affected.[1]

The more ideas in the brain to which to bind the new incoming material, *i. e.*, with which to associate it, the more easily the new matter is remembered. If there is nothing to associate it with the difficulty of remembering is great. This may be tested by giving senseless syllables to be retained or words from a foreign, unknown language. (Great poets, with astonishing literary memories have been abominable spellers. Spelling is learned much as the senseless syllables just referred to, and becomes almost wholly automatic, while literary knowledge is rich in associations.)

That which is associated with emotion[2] (directly accountable for the emotion or associated with it) makes the deeper impression and is retained longer. This is also true of *interest*. The longer the perception lasts, the stronger the stimulus is, or the oftener it is repeated, the profounder is the impression and the more durable the memory. Intensity and extensity (because of newness, bizarreness, largeness, the exceptional, terrible, painful, etc.) influence the durability of memory.

Observation and experiment have also shown that if the thing studied or learned is followed by a pause or rest period, it is retained longer[3] (the retention 'mellows' as it were), while if something directly follows, the retention of the first is weakened.

Again, memory is really composed of many special memories. Some people may have good memories for certain things (names, faces, dates, words, colors, etc.) and very poor ones for other things. In the latter case, the condition may even equal a 'defect.' This must also be borne in mind in examining children. Debile or imbecile children may show the same characteristics—that is, greater ability for certain types of memory, and less ability for other types. Hence, the tests must cover the various types of memory. Only recently Ziehen called attention to this 'partial intelligence defect,' which he says has not been sufficiently noted.[4]

There are still other types of memories characterizing individuals, and which should be thought of, namely, the motor, visual and auditory.

(To be continued.)

[1] Contrary fluctuations within the year (as to seasons) have also been noticed. Thus Schuyter in Antwerp (cited on p. 5 of P. R. Radosavljevich's 'Die Entwicklung des Kindes innerhalb der Schuljahre') found that the months in which the weight increase is smallest are those in which the intellectual growth is least. March, April and May appear to be the months (according to Malling-Hanson) in which the weight and growth are least.

[2] Netschajeff notes (cited by Gaupp) that affectivity in association with words and ideas appears to play a relatively small part between the ages of 9 and 11, but grows rapidly in importance in puberty. In his tests, however, only Russian children were studied.

[3] See R. Gaupp's 'Psychologie des Kindes,' Leipzig, 1910, p. 95. Rest pauses have likewise a very great influence on attention.

[4] Beitrag zur Beurteilung des sogenannten 'Moral Insanity,' etc. *Der Praktische Arzt*, No. 19–20, Leipzig, 1915.

A SUBSTITUTE FOR AN ARTIFICIAL PUPIL

BY C. E. FERREE AND GERTRUDE RAND

Bryn Mawr College

It is a well-known principle of physiological optics that the amount of light condensed into the retinal image of a given test object or source of light varies with the size of the pupil. It is obvious, therefore, that a complete plan or provision for the standardization of the factors which influence the precision of a color determination must take into account the possible sources of error due to a variable pupillary aperture.[1] The remedy usually employed when any account is taken of the factor is an artificial pupil. Whether, however, the use of an artificial pupil is to be recommended is seriously open to question. In our own work we have found its use to be attended by so many difficulties and to be open to so many objections as to be quite inadvisable, unless perhaps in case it is wanted to compare the response to stimuli differing rather widely in intensity. Space can not be taken here for a detailed discussion of these difficulties and objections. It will be sufficient perhaps to say that with the best possible adaptation of the size of the artificial to the natural pupil, its distance from the eye, etc., four difficulties remain which seem inherent and very difficult if not impossible to overcome. (1) The influence of the brightness of the surrounding field on the sensitivity of the retina to the test field can not be satisfactorily controlled.

[1] We will say, however, that we believe this variable error to be very small when a constant light flux is used for stimulus, and a light-adapted eye is employed, working at a moderately high and constant intensity of illumination. We believe this because of the very small mean variations we have obtained in determinations of color sensitivity when all the other factors influencing the response of the eye, which we have discussed in previous papers, are controlled and the pupil is left to regulate itself. In fact we have not been able so far to reduce appreciably the size of this mean error by any artificial regulation of the constancy of the light flux entering the eye from a given constant source. Regulation is desirable, however, when the effect on the eye of different intensities of light is to be compared.

Theoretically considered it would not be possible to attain this control unless the artificial diaphragm could be brought approximately into the plane of the iris. (2) The response of the retina can not be investigated out to the peripheral limits of the field of vision. With the intensity of light attainable with the apparatus described in a preceding paper,[1] red, blue, and yellow can be sensed out to 92° and green to 70° in the nasal meridian. (3) The relation of size of pupil to the cross section of the beam of light which the artificial pupil admits to the eye can not be under observation while the color determination is being made to see whether the regulation needed is actually accomplished in any given case. The adaptation of the size of the artificial to the natural pupil must rest upon a probability established by a number of measurements made on the reaction of the pupil under a set of conditions as nearly as possible identical with those used in the case in question. And (4) the device used to give the pupillary aperture can not be gotten so close to the natural pupil as not to disturb the adjustment of the eye and otherwise to serve as an annoying distraction in the field of vision.

In the apparatus described in the preceding paper the method of presenting the stimulus to the eye is such as to permit of a substitute for the artificial pupil which so far as we are able to determine is entirely free from the objections and difficulties attending the use of a diaphragm in front of the iris. In this substitute plan instead of cutting down the cross section of the beam of light at the eye by means of an apparatus which interferes with the natural functioning of that organ, the regulation needed is accomplished further back in the optical system, out of range of the anterior reactions of the eye and out of the road of the manipulation needed to control the factors which directly influence the response of the eye to its stimulus. That is, the stimulus light is focused by means of the lens (L_2)[2] upon the pupil of

[1] Ferree and Rand, 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral,' *J. of Exp. Psychol.*, 1916, **1**, 247–284.

[2] *Op. cit.*, pp. 254–255.

the eye, forming an image of the analyzing slit of the spectroscope. Obviously the size of this image can be regulated by controlling the height of the slit, for example, or by means of the lens system so as always to fall within the aperture of the pupil reacting to the intensity of the light used in any given case. It adds very much to the precision of the regulation also that its correctness can be checked up if desired for every color determination while the determination itself is being made.[1] That is, not only can the size of image needed for a given intensity of light and set of conditions be determined empirically in a number of trials but its relation to the size of the pupil may be under observation all of the time in a series of determinations of sensitivity. With the lens system and breadth of slit we are now using, we have found that it is not necessary to alter the breadth of the image. This would be done, if it were necessary, by means of an alteration in the lens system. The height is reduced the desired amount by cutting down the height of the analyzing slit which was made variable over a wide range primarily for this purpose, as was stated on pp. 253 and 261 of the preceding article: 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral.' This control of the constancy of the amount of light entering the eye we have found to be entirely feasible and practicable. With it in fact the observation is attended with no more difficulty than if the apparatus were used with no attempt to exercise this control.

[1] Owing to the brightness and sharpness of the image on the cornea with its dark background of iris and pupil, this comparison of the size of the image with the size of the pupil is, it is obvious, not difficult to make. The possibility of having this feature constantly under observation gives this method of regulating the amount of light entering the eye no small advantage over the artificial pupil.

PORTABLE TACHISTOSCOPE AND MEMORY APPARATUS[1]

BY W. F. DEARBORN AND H. S. LANGFELD

Harvard University

The front of the tachistoscope is a brass surface $9\frac{1}{4} \times 4\frac{1}{8}$ inches with a window $2\frac{1}{4} \times \frac{3}{8}$ inches. The window is closed by a paper shutter, which is worked by a brass spring. The speed is below $\frac{1}{10}$ of a second, that is, faster than eye-movements and can be easily regulated by tightening the spring. The shutter mechanism is behind the brass front and entirely concealed from the subject. By pressing the button at *A* the subject can operate the shutter himself or the experimenter can operate it by a string from behind. The brass front is on hinges and can be opened to insert the list of words. A small number is placed to the right of each word in the list and this number can be seen through the small hole *B* at the right of the shutter. It is thus known when the word is behind the shutter and what it is. By leaving sufficient space between the words and indicating the space position by dots on the margin (see sample list, p. 386) the shutter can be closed without exposing the word.

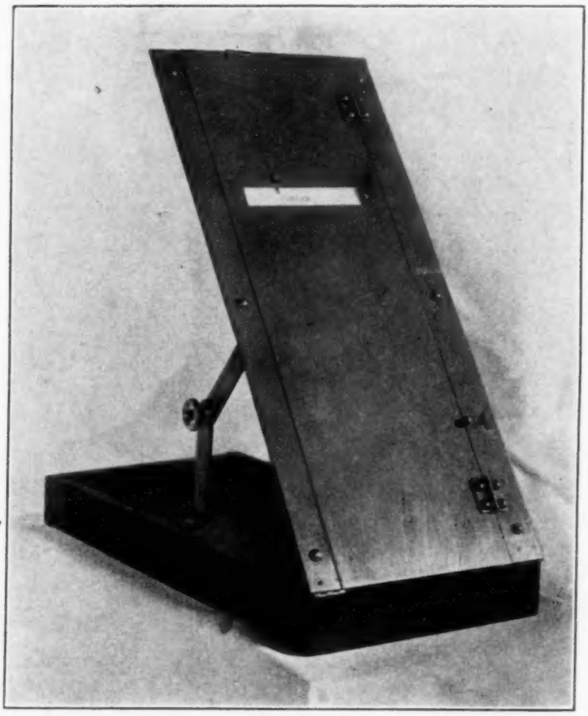
When the window is closed the paper shutter, by sliding between the brass front and a metal contact, opens an electric circuit. The contact is made when the shutter falls and the word is exposed. The exact time of the contact can be regulated by altering the height of the paper shutter.

By half cocking the shutter the window can be kept open. The angle of the brass front to the base can be altered to suit the convenience of the subject and the illumination. When the instrument is not in use the front can be closed, and it then fits into the base so that one has a closed box only an inch in thickness, which can easily be carried in the pocket. The instrument weights 17 oz.

[1] The instrument can be obtained from A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

There is an adjustable brass pointer at *C* which may be used for the purpose of fixation and to direct the attention to the beginning, ending, or various parts of the words.

The instrument meets most of the requirements of an ideal tachistoscope: (1) There is no appreciable change of accommodation, as the point of initial fixation is separated from the word only by the thickness of a thin library card



Portable Tachistoscope and Memory Apparatus.

(paper shutter), a negligible quantity from the ordinary reading distance. (2) The tendency of the eye to follow the moving shutter, which is the disadvantage in most gravity tachistoscopes, is minimized in that the shutter and background are of the same color, and this tendency may be further decreased by enamelling the front surface of the instrument the same shade as the shutter. If this is done the

conditions of ordinary reading of black print on a white surface are more nearly duplicated. (3) The movement of the shutter is sufficiently noiseless not to be disturbing. (4) By reducing the instrument to a very simple form of construction it has been made sufficiently inexpensive to allow its use in quantities in training courses. (5) There is an advantage in the observer being able to release the shutter, as he can thus concentrate his attention on the window at the right moment.

The following are a few suggestions for experimentation, especially in training-courses:

1. As indicated in the sample list, the visual span for objects, numbers, letters, familiar words of various lengths, unfamiliar words, and short sentences may be investigated.

2. Observations may be made in regard to the reading by whole words or parts of words. For this purpose in addition to the unfamiliar words, mutilated words such as 'phylosophical,' and skeleton words such as 'ab-r-vi-t-d' may be used. An example of the reading by parts is as follows:

1st	reading..................	Cyt
2d	"	Cytore
3d	"	l ticum
4th	"	Cytorel
5th	"	Cytore
6th	"	culum
7th	"	ticulum
8th	"	reticulum
9th	"	Cyto
10th	"	Cytoreticulum

3. In order to investigate differences in the form of words, lists of words composed mainly of words with characteristic or determining letters or letter complexes and others composed of the more indifferent letters such as the letters of the line may be used. 'Alphabetical' and 'statistical' are examples of the former, 'conscience' and 'caravan' of the latter.

4. The suggested classification of readers into objective and subjective types may be tested, *e. g.*, by lists of words easily misread. The objective type is supposed to include

readers who have a narrow span but accurate perception, the subjective a wider span but a more suggestive perception. The subjective are supposed to depend in their perceptions more upon the total visual form, the objective more upon the determining or dominant letters.

5. As a test of range of information lists of words from various fields of knowledge may be used.

6. In order to test the influence of a definite mental set two lists may be used, the one composed of relatively unrelated words, the other of words from a specia field.

7. For memory experiments the shutter, as mentioned above, is placed at half cock, which leaves the window open. The lists of words are drawn past the window at a rate regulated by a metronome or sounder in circuit with a clock. The back of the instrument is so constructed that a belt of paper may be used instead of strips, thus obviating loss of time between the successive exposures of the list.

8. The electric contact permits the instrument to be used in reaction experiments by placing the contact in series with battery, chronoscope, and voice key.

N. B. If the instrument is manipulated entirely by the observer, lists may be placed in the instrument with a blank sheet over them and the blank sheet slipped out before starting. This is a useful method in case small classes of pupils in schools are to be tested.

Sample List

* * * *	1
* * * * *	2
* * * * * *	3
* * * * * * *	4
8197	5
68352	6
827153	7
1852379	8

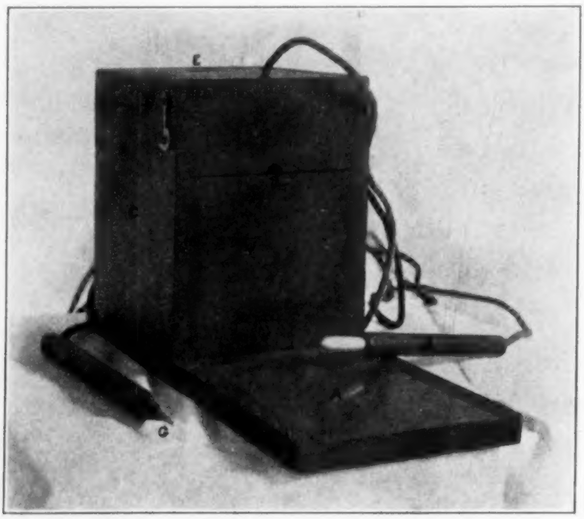
bdcv	9
zjflmt	10
trpbhcd	11
kvnsqgtc	12
watch	13
books	14
wires	15
maple	16
run away	17
how do you do	18
practical	19
multiplication	20
statistical	21
alphabetically	22
caravan	23
environment	24
loquaciousness	25
micromaniac	26
succinamic	27
expansile	28

PORTABLE SELF-REGISTERING TAPPING-BOARD AND COUNTER[1]

BY HERBERT SIDNEY LANGFELD

Harvard University

The instrument consists of a small tapping-board with a brass tapping surface 5×3 inches (*A*), a stylus (*B*), and a box $5\frac{1}{2} \times 4\frac{1}{2} \times 3\frac{3}{4}$ inches (*C*) containing a Veeder counter, magnet, and a five-cell Eveready battery. The board is hinged to the box and can be folded up and hooked (at *D*) to the front of the latter, thus making the instrument very



compact, and convenient for carrying. The board is connected to one of the poles of the magnet through the hinge, and the stylus to the other pole. The magnet, which works the counter by means of a curved armature fastened directly to the shaft of the counter, is quick in action and the fastest

[1] The instrument can be obtained from Mr. A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

tapping can be recorded accurately. The number of taps are read off on the Veeder through a small window in the top of the box at *E*. With this instrument, tests for muscular coördination and fatigue can readily be made in the schools and factories as well as in the laboratory and the results recorded with a minimum expenditure of the experimenter's time.

By substituting a finger ring for the stylus the instrument can be used in finger movement experiments similar to those described by the writer.[1] By a simple adjustment it can be employed to record the free finger movements recommended by Professor Raymond Dodge.[2]

Instead of the tapping stylus a contact pencil (*F*) can be connected to the magnet. This has a metal point (*G*) which, when slightly pressed down, closes a contact in the pencil and actuates the counter. With this pencil, curves, words of a manuscript, etc., can be counted accurately and without mental fatigue.

[1] 'Facilitation and Inhibition of Motor Impulses,' The Psychological Review, Nov., 1915, 22, 6.

[2] 'Psychological Effects of Alcohol.' Washington: Carnegie Institution of Washington, 1915, p. 167.

A NOTE ON FERREE AND RAND'S METHOD OF PHOTOMETRY

BY H. M. JOHNSON

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In a recent communication[1] Ferree and Rand describe a unique method of photometry, which has been in almost daily use in the Bryn Mawr laboratory for four years. In this method, the compared portions of the visual field, instead of being brought as nearly as possible into juxtaposition, have an angular separation of 20° to 25° at the eye. ne of these elements is regarded directly and the other obliquely. The comparison sought is between the amounts of contrast exerted on the compared elements by a campimeter screen lying between them and filling a good part of the visual field. This screen has a higher reflection-coefficient than the compared elements and is illuminated by the same source as are the parts compared. The relatively greater sensitivity of the peripheral retina to the effect of contrast is compensated for by reducing the reflection-coefficient of the compared element which is regarded directly, until the two elements appear to match. The amount of this reduction is taken as an index of the illumination on the campimeter screen. When this appearance of the compared elements under a standard lamp at a known distance is reproduced under a second source substituted for the standard without any change in the reflection-coefficient of the variable element, the illumination on the screen is assumed to be equal under the two conditions; and the luminous intensity of the second source is regarded as computable from its distance from the screen.

The authors propose two conditions to be fulfilled by any satisfactory method of photometry, and submit their method

[1] Ferree, C. E., and Rand, Gertrude: 'A New Method of Heterochromatic Photometry,' JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1916, 1, 1–12.

to test in accordance with them. (1) The method should be sensitive, and its results reproducible; and (2) "it should be known either to possess of itself logical sureness of principle, or its results must agree in the average with those of some method which can be shown to possess this sureness of principle." They claim for their method (1) that with respect to both sensitivity and reproducibility it surpasses the method of direct comparison, or "equality of brightness method," even when the photometer used in the latter method is of the best Lummer-Brodhun type. Applying their criterion (2) they do not discuss the question of whether the method is sound in principle, but they do claim in its favor that the results obtained by it agree very closely in the averages with those obtained by the method of direct comparison, and have an advantage over the results obtained by the latter method in that they are more consistent.

The object of the present discussion is to point out that these claims are not supported by the data which the authors present as proof. For the sake of brevity I shall refer by pages to the original article instead of quoting from it; and the remarks below should be read with the original article at hand for verification.

The authors do not go into details in several important particulars, one being the linear separation of the compared portions of the visual field. However, from the distance given between the eye and the campimeter screen (p. 6) and the angular separation of the compared portions at the eye (p. 4), one concludes that the compared elements were about 10 cm. apart. The vertical plane of the optical bench (photometer bar) was midway between the compared elements (p. 5), and the authors assumed that these elements "received equal amounts of light from the source to be measured." Even if the elements were equidistant from the lamp—as happens not to have been the case (p. 4)—the truth of this assumption does not follow from the data given. In some of the work the results of which are presented in the authors' table (p. 9), the angular separation of the compared elements was 14° to 15° at the source. Now the radiation

from a carbon or tungsten lamp is not equal in all directions, as is that from an ideal point source. In fact, for lamps of such types differences of several per cent. in different directions normal to the long axis of the lamp are the rule, and a considerable difference might occur in a range of 15°. For this reason it is customary for standardizing bureaus to scratch fiducial marks on the bulbs of the lamps which they send out, to enable the experimenter to work in the direction for which the luminous intensity of the lamp has been determined. True, it would have been possible to overcome this difficulty by using as a standard lamp one whose average intensity in different directions in the horizontal plane was known, and by placing the compared lamps in a rotator, so as to utilize their average intensities. As the authors do not mention such a procedure and do not show a rotator in their figure, one would be justified in assuming that the precaution was not taken. It may be well to mention in passing that the likelihood of error from this source would be increased by adopting the authors' recommendation (p. 7, footnote) that the observer be seated farther from the campimeter screen, and the compared elements given a correspondingly greater linear separation.

Due to lack of information from the authors it is impossible to get at the magnitude of these errors, although the probability is that they were not very large, especially with respect to errors from another source which I shall presently indicate. We may therefore drop the former from present consideration.[1]

It should be noted that the plane of the "measuring disc"—a part of which constituted the compared surface

[1] Another source of error which the authors appear not to have taken into account may be worthy of mention. The angles at which the light was diffusely reflected into the eye from the stimulus-patch and the disc at the fixation point were not the same. The percentage of incident light reflected into the eye would have been different, therefore, even if the two surfaces had been of the same material. Furthermore, the difference in percentage of incident light reflected in the direction of the eye is not constant for any two positions of the source. Cf. Wright, H. R.: 'Photometry of the Diffuse Reflection of Light on Matt Surfaces,' *Philosophical Magazine, etc.*, series V, **49**, pp. 199–216. The magnitude of this factor also cannot be estimated in the present case from the data given, but it is probably overshadowed by the other errors.

regarded directly—stood 3 cm. nearer the lamp than the plane of the part of the field with which it was compared (p. 4). The reason for this is not clear. The authors say that it was 'primarily to eliminate any induction by the screen on the disc.' It is not immediately evident how the effect of contrast could be eliminated by placing the test-field and its immediately visible surroundings in slightly different planes; nor is one readily convinced by the evidence which the authors present as showing that this effect was practically eliminated. In fact, it is not even clear why the authors considered it necessary to eliminate it, since the method depends on the difference in sensitivity of the central and peripheral regions of the retina to the effect of contrast. Be that as it may, this disposition of the parts certainly vitiates the assurance given by the authors (p. 5), that 'the contrast-stimulus and the measuring disc thus receive equal amounts of light from the source to be measured.' Neglecting the departure from point-source conditions mentioned above, let us note the effect of this difference in distances on the results as presented in the authors' table (p. 9).

The data presented in the third column give the 'distance of white light [source] giving equality of illumination' as compared with certain lamps at given distances with colored filters interposed, as determined by the authors' new method and by the method of direct comparison ('equality of brightness method'). The authors do not describe their mode of procedure in making their measurements by the method of direct comparison. I assume, therefore, that they used the standard procedure, in which the 'test-lamp' and the standard lamp are successively placed on one side of the photometer head, a third lamp being kept at a fixed distance on the opposite side; the whole system being mounted on an optical bench, and suitable protective diaphragms being interposed to exclude extraneous light. Under these conditions, and working with the lamps beyond certain minimal distances from the photometer-head, the luminous intensities of the compared sources would be inversely as the squares of their distances from the photometer screen at valid settings for

equality of brightness on the two halves of the photometer field. Assuming that this set of measurements was made in this way, let us examine a little more closely into the comparison between these results and those obtained by the new method, according to the authors' table.

It appears from these data that the average settings by the new method are very consistent, and that they agree within a fraction of one per cent. with the average settings made by the method of direct comparison, in which the inverse square relation may be supposed to hold. This agreement is hard to understand, in the light of facts derived from the authors' own data, and presented in the accompanying table.

Source	Filter	Colored Illumination			Colorless Illumination Giving Same Appearance of "Contrast Stimulus" and "Measuring Disc"			Deviation from Proportionality of Illumination, Per Cent.
		Distance of Source from Plane of Screen	Distance of Source from Plane of Disc	Illumination on Disc Behind Fixation-Point, Per Cent. Illumination on Screen at Stimulus Opening[1]	Distance of Source from Plane of Screen	Distance of Source from Plane of Disc	Illumination on Disc Behind Fixation-Point, Per Cent. Illumination on Screen at Stimulus Opening[1]	
87 c.p.	Red	41 cm.	38 cm.	116.1	66.6 cm.	63.6 cm.	109.6	6.5
" "	Blue-green	41	38	116.1	59.5	56.5	110.7	5.4
52 "	Red	38	35	117.5	82.2	79.2	107.4	10.1
" "	Blue-green	38	35	117.5	70.5	67.5	109.0	8.5
13 "	Red	38	35	117.5	160.0	157.0	102.0	15.5
" "	Blue-green	38	35	117.5	134.9	131.9	104.5	13.0

In brief: by the authors' method, the stimulus-patch and the measuring disc were not equidistant from the lamp, and consequently received unequal illumination from it. The difference in illumination being approximately the difference between the reciprocals of the squares of the distances of the illuminated surfaces from the lamp, it was not proportional for any two positions of the lamp on the photometer bar. When the colored sources were used, the compared elements

[1] Assuming applicability of inverse square law, and taking as distance between source and illuminated point the hypotenuse of a right triangle of which one leg is 5 cm. (the distance of the stimulus opening and the fixation point from the axis of the optical bench) and the other leg the distance of the source from the plane of the screen, or disc, as the case may be.

appeared to match when the difference in illumination was 16 per cent. to 17.5 per cent. in favor of the disc. When a colorless source was used instead of the colored source, and placed at the proper distance to give the same illumination on the screen as the latter received from the colored source,[1] the patch and the disc presented a satisfactory match when the reflection-coefficient of the disc was the same as under the colored lamp. If the method were valid, we should expect a match to hold only when the same proportionate difference in the illumination on the patch and on the screen existed as in the former case. Nevertheless, instead of the difference of 16 per cent. to 17.5 per cent. which obtained at a match under the colored source, the difference under the colorless source was only 2 per cent. to 10.7 per cent. The discrepancy, as shown in the last column of my table, varies between 5.4 per cent. and 15.5 per cent. Thus the agreement in settings made by the authors' method and by the method of direct comparison would appear to be spurious evidence of agreement between the two methods, and good evidence that the authors' procedure in making the settings was faulty.

The authors give no information which indicates where the trouble lies, nor do they appear to have noted the discrepancy. In an ordinary situation of like kind one would suspect that the settings made by one method were influenced by knowledge of the settings made by the other method. I do not wish to be understood as offering that as an hypothesis in the present case, however; nor do I wish to divert the discussion from the main issue by proposing any other hypothesis.

To conclude: the data presented by the authors, taken at face value, indicate that the method is insensitive, since large deviations from proportionality of difference in illumination on the compared portions of the field were not detected by it. While the authors' data indicate that the settings made by their methods are highly reproducible, the thing reproduced is an error the magnitude of which is large, and

[1] As determined by the method of direct comparison, in which the inverse square relation may be assumed.

not even a function of the illumination received from the compared sources. The authors do not claim soundness of principle for their method, and their claim that its results agree with those obtained by a sound method is seen to be erroneous.

Unpromising as the method may appear in principle when considered *a priori*, it would be unscientific to condemn it without a fair and thorough trial, which it apparently has not yet received at the hands of its originators.

EXTRAVAGANCES IN THE MOTOR THEORIES OF CONSCIOUSNESS

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The biological point of view in psychology has brought the so-called motor theories of consciousness into sharp relief. As yet there has been no formulation of these theories into a coherent whole. They represent a tendency in the thinking of today and not a consensus of opinion. The disposition to interpret consciousness in motor terms is not new; it may be found in many of the earlier writers of modern psychology who sought explanations in the motor factors for various aspects of consciousness, such as attention, space-perception, ideo-motor actions, emotion, and volition. Today the more ambitious program would include all aspects of consciousness. Broadly stated, all consciousness is conditioned or accompanied by motor activity. Such assumptions may be detected in much of the recent work in psychology. It is difficult, however, to find completely rounded out theories. Of these the most influential are probably those of Dewey, Münsterberg, Judd, and Watson.

Chronologically Dewey[1] stands in the forefront. His conception of the reflex arc as converted into a circuit laid great stress on the motor side of processes that were commonly considered sensory. The sensory-motor processes are all one process in this scheme. "It is just as true to say that the sensation of sound arises from a motor response as that the running away is a response to the sound." The reason for this lies in the fact that "the sound is not a mere stimulus or mere sensation; it again is an act, that of hearing. The muscular response is involved in this as well as sensory stimulus. . . . The movement and posture of the head, the tension of the ear

[1] J. Dewey, 'The Reflex Arc Concept in Psychology,' Psychol. Rev., 1896, 3, 357-370.

muscles, are required for the reception of the sound. . . . The conscious sensation of sound depends upon the motor response having already taken place. . . . Indeed, the movement is only for the sake of determining the stimulus, of fixing what kind of stimulus it is, of interpreting it."

Dewey's concept holds sensory, central, and motor processes as coördinations of one process. This would certainly imply that the motor elements were indispensable in consciousness.

Münsterberg's[1] 'Action Theory' maintains that every sensation and every element of consciousness is dependent on the passage of stimulation into discharge in the cortex of the brain. The quality, intensity, and vividness of sensations depend upon the parts of the cortex affected and the character of the current passing through to the motor tracts. In a recent formulation of his theory[2] he declares that whether a color or a noise become vivid in the mind or are refused admittance depends upon the conditions which prevail in the motor centers. If the channels of discharge are closed the sensations will be inhibited. "According to the popular view a world of impressions and ideas exists in us, entirely independent of our actions; and when they are complete and perfect they send their message to some motor apparatus which carries out the order. Such a fancy must be entirely reversed. In every moment the motor situation decides the possibilities in the sensory sphere. Our ideas are the product of our readiness to act." Whether the author means ready to act or action is not clear, for in another place[3] he declares: "The centripital function appears to be almost more important than the centrifugal one which bears the stimulus. It is the action of the organism which controls the mental life." It would seem from the following that an actual movement were necessary: "The personality's thinking is as much the product of his actions as his actions are the product of his thought."

In Judd's psychology[4] the motor theory is everywhere

[1] H. Münsterberg, 'Grundzüge der Psychologie,' Bd. I., S. 548.

[2] 'Psychology, General and Applied,' 139–144.

[3] *Op. cit.*, 423.

[4] C. H. Judd, 'Psychology,' 71, 134, 296, 183.

present. In an early chapter he states, when analyzing consciousness: "In some form or other, every incoming sensory impulse and every central nervous process must issue in a motor discharge. It is equally true, though by no means so obvious, that every form of conscious experience is related to behavior." In discussing sensations he says: "Our problem is not exhausted, however, when we have dealt with the interrelation between sensations, for every sensation is related in some way to the active processes of expression." In the complex associational processes there may be a delay of minutes, or days, before the excitation is "discharged through the motor centers in the form of a reaction." His conviction that "there is a universal relation between consciousness and bodily activity," rests upon biological grounds. "The structure of the nervous system from the hydra to man is such that there is always a motor organ linked with every sensory organ."

With Watson[1] we come to the logical culmination of this trend of thinking. "The nervous system functions in complete arcs. An incoming impulse exerts its effect relatively immediately upon one system of effectors or another." So precise is this relation that, "in a system of psychology completely worked out, given the responses the stimuli can be predicted; given the stimuli the responses can be predicted." The responses may be 'explicit' or 'implicit'; in the latter case they involve chiefly the actions of speech mechanisms. If these could be detected and recorded the material which the introspectionist claims as peculiarly his own would be accessible to the behaviorist, and the thought processes would be interpretable in terms of their motor accompaniments. So confident is Watson in the principle of the complete arc functioning in thought and speech that he declares: "If we could find a case where a man suddenly lost his laryngeal apparatus without any serious injury to the bodily mechanisms, we should have a crucial case. From our point of view there would, or ought to be, a serious limitation in this man's thought processes."[2]

These brief citations do not do justice to the author's

[1] J. B. Watson, 'Behavior; an Introduction to Comparative Psychology,' Chap. 1.
[2] *Ibid.*, 327.

theories in their entirety; that would be impossible in a short article. Nor is it the present purpose to discuss these theories. The issue in hand is to point out an obvious extravagance running through this trend of thinking; namely, the insistence that a motor expression accompanies *all* conscious processes. No one will deny that there is a deep-seated tendency for the incoming impressions to go out into motor expressions; but there is nothing more than a *tendency.* The authors quoted above, and a number of others who have written less fully and frankly, push this principle to an extreme. They are seeking to make it an open-sesame into the most intricate problems in psychology.

To put the most obvious criticism into logical form, the champions of the motor theories commit a fallacy in generalization. They infer of a whole class what we know only of a part.

Moreover, they are captivated by the principle of continuity. The structure and the function of the nervous system in its evolution do undoubtedly show the intimate relation of receptor and effector, of stimulus and response. It is very easy to assume that a principle which is corroborated so frequently in the animal scale should apply to the most intricate type of nervous system. But does it? Facts alone can answer the question.

The influence of biology is clearly discernible in these theories. The psychologist who thinks of the nervous system in terms of the hydra may readily imagine every stimulus effecting a prompt response, but one who thinks in terms of human anatomy can hardly do so, for the anatomist himself is too circumspect. Villiger,[1] for example, insists that "only after traversing numerous neurones does the impulse finally reach the motor center and from there pass to the motor path, since coöperation of the various cortical centers must be assumed in explanation of the complex psychic processes." If this passage were by simple nerve trunk conduction every stimulus might go out into a response, but it is not so. A series of obstinate neurones and synapses must be traversed in the cortex and an excitation may die out anywhere along the

[1] E. Villiger, 'Brain and Spinal Cord,' 133.

line. Sherrington[1] shows that the reflex chain of several neurones offers higher resistance than that of a few neurones. What would happen if an impulse stalled in some high-threshold synapse before reaching a motor tract? Nothing, says the motorist; it must cross the mystic line (Münsterberg); it must make the round trip (Watson). Only when it crosses the frontier and enters the motor tract is it given a passport into consciousness.

If this be so, then the motor areas are most important for consciousness. Injuries in them should do greater damage to consciousness than injuries elsewhere. Southard[2] finds the reverse to be true. Injuries in the posterior areas cause greater disturbances than those in the anterior. Cushing[3] found that stimulating sensory areas did give an effect in consciousness, but the same stimulation in motor areas did not. Even if we ignore the cortex and consider the structure of the afferent and efferent tracts we can find nothing to support the motorist's ideal. According to his schema the two sets of tracts have equal tasks, they are parts of one circuit, or process, the motor being as active and important as the sensory. Anatomically, nothing of the kind exists. The sensory tracts outnumber the motor by three or four to one, and there are more neurones in the former than the latter. Moreover, when we compare the complexity of the sense organs with the simplicity of the muscles and glands the disparity is overwhelming. Washburn's[4] contention that "every sensation that can be discriminated in a fusion, and every group of sensations that can be attended as a single whole has connected with it one or more movements which are peculiar to it alone," calls for a much more elaborate motor apparatus than nature supplies. It is inconceivable that each fiber in the basilar membrane, affording over twenty thousand discriminable tones, should have its accompanying little movement with a meaning all its own. Indeed, if any inference is to be drawn from the structure of the human animal it must give a greater prominence and

[1] C. S. Sherrington, 'The Integrative Action of the Nervous System,' p. 156.

[2] E. E. Southard, Psychol. Bull., 1914, 11, 66.

[3] H. Cushing, *Brain*, 1909, 32, 44–53.

[4] 'The Function of Incipient Motor Processes,' Psychol. Rev., 1914, 21, 376–390.

importance to the sensory organs, tracts, and areas than to the motor. The two systems are not on a parity. Nothing but the exigencies of a theory could assign them equal duties in conscious processes.

Nor are the motor theories in better case when we turn to experimental results for evidence of invariable response to stimuli. Truly, many organic changes accompany various conscious states, but a close correspondence of the one to the other is far from proved. Shepard[1] in summarizing the results of work in this field says: "No factor, vasomotor, rate or amplitude of pulse, position or emphasis of dicrotic, or rate or amplitude of breathing, changes one way for agreeable and the opposite way for disagreeable conditions. . . . The rate of the heart is sometimes increased on the average, sometimes decreased, sometimes not changed at all. . . . The rate of the heart with sensory stimuli increases about as often as it decreases. Mental application gave a slight amount of vasomotor change, frequently so slight as to be hardly noticeable. . . . In about half of the mental application tests there are both vasodilation and vascconstriction in a single test. When the change is only in one direction it is an even chance that it be dilation or constriction." Surely, no one would venture to predict the stimuli, given the responses, in that mêlée of confusions; and only the hardiest of theorists would seek to demonstrate how the organic movements determine the conscious states from that material.

The experimental results bearing upon the relation of unconscious movements to impressions and ideas are also inadequate to the motorist requirements. Stein[2] found that it was possible to educate many subjects to make automatic movements. However, there was a large percentage of both males and females from whom she could get no automatic movements at all. The so-called ideomotor reactions are nothing more than habits. They do not rest upon any physiological principle similar to that of reflexes, or instincts. They grow up out of experiences as do all associations. As we

[1] *Amer. J. of Psychol.*, 1906, 17, 522–584.
[2] Psychol. Rev., 1898, 5, 295–306.

habitually associate the words 'cat' and 'dog,' 'pretty' and 'girl,' so too do we associate an oncoming baseball and the catcher's movement, or the threatened blow and the pugilist's 'on guard.' Many of us accompany our language with gestures. These easily become habitual and accompany certain thoughts which may or may not be expressed in words. This conception of ideomotor action is supported by a study of the involuntary movements of children and adults.[1] Among children there is less directness in such movements than in adults. In imitating unconsciously the movement of a moving object they are less susceptible than adults. They show less fixity than adults in both their movement and verbal associations. It is habit, extending over a period of years, that gives permanency to associations. There is nothing in the oft-quoted ideomotor actions to warrant the assumption that every incoming impression goes at once into some form of movement, and that this is based upon the structure of the nervous system.

In speech-motor reactions it is probable that the same habit principle obtains. Curtis[2] fastened a tambour to the larynx in such a way as to record slight movements there. He could obtain records when his subjects read silently or recited mentally. Nevertheless, twenty per cent. of his subjects gave no records of any movements.

The motorist is not on safe ground when he retreats to the position that impressions *eventually* issue into action. This assumes that an impression may persist for a long time and then assert its motor rights. Some impressions may do that. Many of them fade out, merge into a type or into some other impression. We can no longer hold the fantastic belief that every impression leaves an indelible mark on the soul. The few striking instances of memories evoked in delirium, or hypnosis, which bring forth events forgotten for years, do not prove a miraculous retention on the part of the cortex. If every impression goes forth into expression, then each tree, twig, stone, pebble, sparrow, and cloud that flits past the car window from Boston to San Francisco will leave a mark on the obser-

[1] M. A. Tucker, 'Comparative Observations on the Involuntary Movements of Adults and Children,' *Amer. J. of Psychol.*, 1896–97, **8**, 394–404.

[2] *Amer. J. of Psychol.*, 1900, **11**, 237–239.

ver's nervous system which will later issue in some form of behavior! This *must* happen despite the fact that the observer forgets these trifles as quickly as he sees them!

A less fantastic and more important feature of the motorist's program seeks to maintain the doctrine that conscious states are conditioned, or determined, by motor activities. This does not mean that the kinesthetic sensations of movements accompanying perceptions blend with these perceptions and give a certain character to them. That is an old, an obvious fact. The conception is derived from the belief that sensory, central, and motor processes are one process. All three function together. It is an extreme statement of the unity of the nervous system and of conscious activities. Its over-emphasis distorts a valuable truth and a serviceable conception.

The strongest arguments for the contention are to be found in the relations between the thought and speech processes. So intimately is speech bound up with ideation and, indeed, perception that it would seem impossible to separate them. If the motor discharge conditions the conscious content in any activity it must do it here. If the motor areas are indispensable to the neural basis of any conscious state, they are indispensable here. This is so apparent that Watson pins his faith to the integrity of the process. A man lacking speech-motor action is a 'crucial case' for behaviorism. Watson believes that such cases are 'extremely unlikely' to occur; they are rare, but they do occur. Pure motor aphasia calls for a disorder in such a minute and restricted area that few cases are recorded. Usually the disturbance involves other parts and a number of sensory and motor defects accompany the aphasia. Rare though such cases are, they are sufficiently well known and adequately described for the purpose in hand. Dejerine[1] states that "the sole phenomenon consists in the impossibility of the articulation of sounds in all their modes, but all other qualities of language are intact and the interior language functionates as in the normal individual." Moutier says the condition is one in which the patient finds it impossible to communicate with his entourage, the tongue does not obey

[1] Quoted by Dercum, *J. of Nerv. and Ment. Disease*, 1914, 41, 137–141.

the will. The thoughts in his mind are very clear and his words precise, but he cannot exteriorize them at all. "There is present but one symptom and that is the suppression of articulate language." Dercum describes such a case. His patient showed no sensory losses at all. "He had apparently complete motor aphasia. He was unable to make the slightest articulate sound. . . . At the same time the patient's understanding of spoken speech and comprehension of what was said to him was perfect. . . . Speech comprehension was completely preserved. Further, the patient could read written or printed matter and carry out instructions perfectly." There was no agraphia, he could write spontaneously and from dictation. Dr. Dana[1] reports three cases on his lists. Of one he says: "The patient cannot say a word, or repeat, or ejaculate, or count. But he writes, reads, understands, and has good general intelligence." In short, here we have several cases of the loss of the speech mechanisms and no 'serious limitation' in the patient's 'thought processes.' The elimina, tion of the motor factor has not disturbed the central and sensory processes. The perception of words has not been impaired, much less inhibited, by the inability of the incoming impulse to discharge into the speech motor area.

If the motor discharge into the larger musculature of the trunk and limbs were necessary to normal conscious life we should expect to find curious distortions in the thought and feelings of anyone suffering from a motor paralysis. Indeed-from the certainty which runs through the motor theories, we should be able to map out definite fields of consciousness which would be impaired as a paralysis spreads over legs, trunk and arms. As a matter of fact nothing of the kind occurs. Cases may be found where only the spinal motor tracts are involved. In these there is no obliteration of large parts of the thought and emotional life of the patients—certainly no impairment of perception.

It seems probable that the line of attack will be shifted by the more ardent of the motorists from overt movements of the musculature to less accessible regions, where observation

[1] *N. Y. Med. Journal,* Aug. 10, 1907.

and experiment cannot follow. The glands offer a tempting opportunity. If we *must* have every incoming impulse and every central nervous process issuing in a motor discharge, why not assign the splendid duty to that famous and historic organ, the pineal gland?

DISCUSSION

FURTHER LOGICAL ASPECTS OF THE BINET SCALE

BY TRUMAN L. KELLEY

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Due to what the writer considers is the substantial progress made by Otis[1] toward the development of a theoretically sound procedure in standardizing and using tests of the Binet type, as opposed to the altogether empirical, not to say haphazard, methods characteristic of past work along this line, he feels that a criticism and supplementing of Otis's conclusions may be helpful.

Otis concludes with reference to purpose 1 that a test is standard for the age at which 50 per cent. pass the test. His summary, paragraph 2, is a logical sequence to this definition of "standard."

"If tests are grouped together in age groups such, for example, that the tests in age group X. are standard for ages ranging from $9\frac{1}{2}$ to $10\frac{1}{2}$, or such that the number of tests standard for ages above ten is probably equal to the number standard for ages below ten (as would be the case by chance if the tests were placed roughly as near ten as possible), or if the tests were all standard for exactly the age of ten, then the proper way to score them would be to give the child tested a score of ten years in mental age if the number of tests passed equalled the number to and including one half of the ten-year tests, and the same for the other years. If, however, the tests are to be scored as at present, giving a score of ten years of mental age only when all the tests to and including those in group X., or an equivalent number, are passed, then group X. must be composed of tests standard for ages ranging from nine to ten, or all standard for nine and a half."

This entirely sound procedure is quite at variance with the practice of the devisers of tests of the Binet type who have used 75 per cent. correct of the responses of a given age as a criterion for considering the test as standardized for that age. Otis explains the fact that their results have been at times approximately correct

[1] Arthur S. Otis, 'Some Logical Aspects of the Binet Scale,' Psychol. Rev., 1916, **23**, Nos. 2 and 3.

as due to the fact that these workers had inadvertently lost track of a half year in making their calculations; in brief, that they had assigned tests passed by 75 per cent. of children of average age 10.5 to a group the passing of which gave the child credit for mentality 10.0. So obvious an error is hardly to be attributed to an oversight on the part of the experimenters concerned. If they did resort to the procedure charged we may well believe it was because the empirical results obtained were more in harmony with the known mentality of the children tested than would have been the case by adding .5 of a year.

It is not argued that their procedure was logically sound, that would be maintaining more than in all probability even they would claim, but it is proposed to show that to secure a group of five tests the passing of all of which is to be expected from 50 per cent. of children of age 10.0, does require that each of the tests separately be passed by some per cent. greater than 50.

If there are five tests *a*, *b*, *c*, *d*, *e* for age 10.0 and if each of the tests separately is passed by 50 per cent. of the children of this age, then only in case every child who passes test *a* also passes each of the others and vice versa does it result that 50 per cent. of the 10.0-year-old children pass the entire group of five tests. Only in case the correlation between each of the tests in the group and all the others is perfect, or 1.00, does 50 per cent. passing for each test singly result in 50 per cent. passing the entire group. A group of tests perfectly correlated would have no significance over and above a single one of the group, for the one test alone would divide the individuals into exactly the same divisions as would the entire group.

If the five tests are each passed by 50 per cent. of a given age and if each test is uncorrelated—shows .00 correlation—with each of the other tests then out of a number taking test *a* 50 per cent. will pass; out of the 50 per cent. that pass one half, or 25 per cent. of the total will pass test *b*; etc., until we have $3\frac{1}{8}$ per cent. only passing all five tests. This result may be obtained more readily by simply evaluating $(\frac{1}{2})^5$.

If we have five uncorrelated tests and wish to find the per cent. that must pass each separately if 50 per cent. are to pass the group entire, we may do so by means of the equation, $x^5 = .50$. Solving $x = .8705$. Omitting consideration of the relationships that would maintain if some of the correlations between the five tests were negative, for it is hardly likely that such would ever be the case,

it is apparent that for five tests as a group to be passed by 50 per cent. of the children of a given age, some per cent. greater than 50 and less than 87 must pass each of the tests separately, the exact per cent. needed being dependent upon the intercorrelations between the five tests—the greater these correlations are the lower the requisite per cent. and vice versa.

These considerations show the futility of attempting to establish a single per cent. which can be reliably accepted as standardizing a test if that test is to be grouped with others to yield 50 per cent. of passes for the entire group. Any per cent. established would be true in case certain intercorrelations held, and as these intercorrelations are by no means constant a single per cent. cannot be established as the standard. Suppose that it had been determined, as it might be by means of very laborious calculations, that five tests, each showing the same definite correlation *k* with each of the others, must separately be passed by 75 per cent. of children of age ten in order that 50 per cent. of such children pass the entire group. Suppose further that this 75 per cent. criterion is used to put five tests showing mutual intercorrelations of less magnitude than *k* into a single age group. The result will be that less than 50 per cent. of the children of the age considered will pass the group entire and accordingly the group is too hard. This is probably the situation that actually maintains in the 11 and 12 year Binet groups and in part accounts for the fact that exerimentally they have been found to be too difficult, though from the standpoint of percentage of children passing each test they are standardized upon the same basis as the tests in other age groups.

A very simple way to circumvent the difficulties mentioned is to standardize tests upon the basis of 50 per cent. correct responses by children of the age for which standardized and then give a child credit for this age if he passes 50 per cent. of the tests in the group. (Otis reaches this same conclusion by a somewhat different approach.) With such an arrangement no systematic tendency to pass more or less than 50 per cent. of normal individuals of the age for which the tests were standardized would result as a consequence of high or low intercorrelations between the tests in the group. Though no such constant error would be introduced by having a group of very highly intercorrelated tests, such tests would result in mental age determinations having a large probable error. For these reasons and others[1] adopting 50 per cent. right answers

[1] I have shown in "A Simplified Method of Using Scaled Material," *School and*

by children of a certain age as the criterion for acceptance of the test for that age is to be recommended. The facts just presented lead to a second criticism of Otis's recommendations.

If there are five tests, standardized as recommended, for the age 10.0 group and individuals who pass 0, 1, 2, 3, 4, 5 of them respectively are considered to be successively of higher mentality, a distribution like that shown in Fig. 1 would represent the situa-

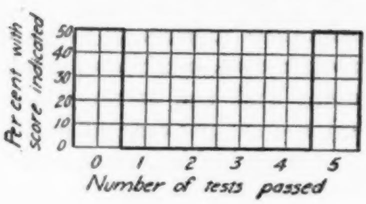

FIG. 1.

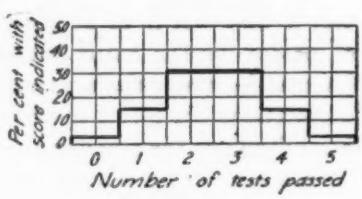

FIG. 2.

tion in case the five tests are perfectly correlated and are given to a number of children of mentality 10.0. There would be 50 per cent. of complete failures and 50 per cent. of total successes, giving an average success of $2\frac{1}{2}$ tests, or 50 per cent. right, and the standard deviation of this distribution would equal the distance covered by $2\frac{1}{2}$ tests which would be .5 of a year if the age range to which the tests were applicable was one year. On the other hand, given a zero correlation the distribution would be as shown in Fig. 2. The average accomplishment is the same as before, but the standard deviation of the distribution is only 1.118, or .2236 of a year, showing much greater reliability in this case than in the former. It may be stated[1] that if the five tests show some positive intercorrelation greater than 0 and less than 1, the standard deviation of the corresponding distribution would lie between .5 and .2236, the smaller the intercorrelations the closer to .2236. These considerations aid in establishing the criteria as to what constitutes a good group of five tests.

Otis proposed and answered the question as to what constitutes a good test, but in doing this he assumed that he had at the

Society, 1916, 4; 79–80, that the determination of the difficulty of questions such that a child can pass 50 per cent. of them is more reliable than the determination of a difficulty corresponding to some other per cent.

[1] The rigorous proof of this involves the calculation of the standard deviations of correlation solids of five independent variables. The writer has not attempted this very laborious task but he has established the principle for the case of two independent variables.

same time laid down the criteria for determining the best tests when used as a group. He is in error in this and for fundamentally the same reason that he was in error in his understanding of why tests standardized upon the 75 per cent. basis do give, when used in a group as in the Binet test, approximately 50 per cent. of passes for the group of five. The problem at hand is one which is, in the statistical sense, best solved by partial correlation and not by total correlation.[1]

Otis recognizes that the determination of the merit of a group of tests must rest upon their relationship with an outside standard. Having then established by as reliable means as possible, other than by means of the tests themselves, the degrees of intelligence possessed by a number of ten-year-old individuals, a large number of tests may be given to these subjects for purposes of standardization. The scores made by each of the ten-year-olds, for any five of these tests may be correlated with the established measures of intelligence for these same individuals and that group of five which yields the highest correlation will be the best group. This procedure is fundamentally different from that of determining the merit of a group by means of the individual correlations of its parts with established measures of intelligence. Unless the intercorrelations between tests are taken into consideration each test of the five will likely measure the same, or nearly the same, trait—some trait which correlates highly with the established measure of intelligence—and the group of five will fail to measure the all-around mental make-up of the individual. The procedure proposed has been with reference to securing the best group of five tests for a given age. In actual practice it would be better to extend the group to include all tests likely to be given to a single age.

In giving the requirements of a test the present writer would supplement, by the addition of the words in italics, the statement made by Otis on p. 166:

"Thus, if 50 per cent. of ten-year-olds pass a test, the degree of merit of the test as a test of intelligence, *when used in conjunction with other tests*, is measured by the degree in which those who pass it are identical with those whose intelligence is known to be above ten-year intelligence *and also by the smallness of its correlations with the other tests with which it is to be used*."

[1] The value of this method has been more fully exemplified in Truman L. Kelley, 'Educational Guidance,' 1914, where the grades of several tests have been combined into a single grade which resulted in the highest possible correlation with the trait dealt with.

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